STATE OF MINNESOTA DISTRICT COURT

COUNTY OF RAMSEY SECOND JUDICIAL DISTRICT

- -

The State of Minnesota,
by Hubert H. Humphrey, III,
its attorney general,
and
Blue Cross and Blue Shield
of Minnesota,

Plaintiffs,

vs. File No. C1-94-8565

Philip Morris Incorporated, R.J.
Reynolds Tobacco Company, Brown
& Williamson Tobacco Corporation,
B.A.T. Industries P.L.C., Lorillard
Tobacco Company, The American
Tobacco Company, Liggett Group, Inc.,
The Council for Tobacco Research-U.S.A.,
Inc., and The Tobacco Institute, Inc.,

Defendants.

- -

DEPOSITION OF DONALD B. RUBIN

Volume III, Pages 475-737

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(The following is the Deposition of DONALD R. RUBIN, taken pursuant to Notice of Taking Deposition, at the offices of Robins, Kaplan, Miller & Ciresi, Attorneys at Law, 222 Berkeley Street, Boston, Massachusettes, on April 13, 1998, commencing at approximately 9:38 o'clock a.m.)

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E X A M I N A T I O N I N D E X

WITNESS	EXAMINED BY	PAGE
Donald B. Rubin	Mr. Love	480

E X H I B I T I N D E X

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P R O C E E D I N G S

(Plaintiffs' Deposition Exhibit 3546 was marked for identification.)

(Witness sworn.)

DONALD B. RUBIN

called as a witness, being first duly sworn, was examined and testified as follows:

ADVERSE EXAMINATION

BY MR. LOVE:

Q. Professor Rubin, I just introduced myself to you a couple minutes ago but I'll repeat that I'm John Love, I'm with the law firm of Robins, Kaplan, Miller & Ciresi, and as you know, our firm represents the State of Minnesota, Blue Cross and Blue Shield of Minnesota in the tobacco litigation that's currently ongoing in Minnesota against various tobacco companies and industry groups. You are familiar with this litigation generally?

A. Yes, I am.

Q. And you were deposed earlier in this case?

A. Yes.

Q. October 6 and 7, I believe?

A. The dates I don't remember exactly but it sounds about right.

Q. You understand this is a continuation of your

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deposition from before?
A. Yes, I do.
Q. And that all the rules about depositions that applied to that deposition apply to this deposition as well?
A. Yes, I understand that.
Q. And just as before, it's important for you to give an audible answer to all of my questions so the court reporter can take it down, nods of the head and so forth really he can't record accurately.
A. I understand.
Q. If I ask you any questions today you don't understand, please let me know and if you answer the question, I will assume that you understood the question. Is that fair enough?
A. Yes, that is.
Q. If you need to take a break at any point during the deposition today, just let me know and we will certainly accommodate you.
A. Okay. Thank you.
Q. Professor Rubin, I'll show you what we have marked as Plaintiffs' Exhibit 3546, titled Supplemental Expert Report of Donald P. Rubin, Ph.D., January 10, 1998.
A. I hope it says B. Rubin.

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Q. I'm sorry, did I say P?
A. You did.
Q. It does say B.
MR. BIERSTEKER: It's a little early and it's Monday. Thanks.
Q. Professor Rubin, is that an accurate copy of your supplemental expert report in this case?
A. Glancing through it, it looks that way.
Q. You don't recall there being any attachments to it or so on that aren't included there?
A. I'll take a slightly more careful look.
Q. Sure.
A. No, I believe this is everything.
Q. If you will excuse me one second.
(Discussion off the record.)
BY MR. LOVE:
Q. Professor Rubin, I'll show you what was previously marked in your earlier deposition as Plaintiffs' Exhibit 3540, now marked as trial Exhibit 2273, Report of Professor Donald B. Rubin in the Minnesota Cigarette Litigation, July 19, 1997.
A. Okay.
Q. That's your original expert report; correct?
A. It certainly looks that way, although I do think there were some attachments. There was an attachment

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to this maybe that's not here.
Q. Let me show you what was marked as Exhibit 3544 to your first deposition, it's also marked trial Exhibit 2277, a memo to you from T. E. --
A. Raghunathan.
Q. Raghunathan. Is that what you were referring to that was attached?
A. No. I believe in the original report there may have been a figure for the missing data part. Attached hereto as Exhibit 2 is a computer diskette containing a simulation -- I guess an industry report was attached, it's a diskette. That's why it's not on paper.
Q. All right.
A. Okay.
Q. I'll show you one more document that was marked as your original deposition, it was marked as exhibit -- Plaintiffs' Exhibit 3545, it's also marked trial Exhibit 2278. Do you recall that from your original deposition?
A. Yes, I do. Yeah.
Q. Other than the computer diskette, are exhibits 34 -- 3545, 3544 and 3540 the work product that you produced prior to your supplemental report?
A. Yes.

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Q. And do these four exhibits that we have just identified contain all the expert opinions that you have reached in this case?
A. I don't think so. There certainly are other opinions that I have that are probably not summarized in these documents.
Q. All right. Please tell me what other opinions you have reached in this case as an expert.
A. That's probably easier to come up with in response to specific questions. I guess my answer just is, in saying everything I've thought about in the case and all the documents I've read is not summarized under these few pages.
(Interruption by the reporter.)
A. Are not summarize under these few pages that are given here. If you are saying are there any other formal opinions that I have written down and distributed to people, I believe this is the collection I've written down. Certainly there are many more things in my head that I've thought about that aren't written down here, I believe.
Q. Have you reached any other expert opinions that you intend to describe to the jury during the trial of this case if you are called as a witness?
A. In response to questions, it's possible. If

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somebody asks a question that involves something that's other than summarized here, then I would respond.

Q. Can you give me any examples at all?

A. Since the last document, which I think is the supplemental report, I certainly had been asked to read a variety of documents. Even in preparation for this deposition I think I was asked to read 10,000 pages in one-day's notice, approximately, for this deposition. Obviously I didn't do all of that but there are things that I've been asked to read since January 10 and so I probably have opinions about those documents that aren't summarized in a document written before that date.

Q. Other than your opinions about other documents that you have looked at, have you formed any expert opinions of your own besides those you have expressed in the four exhibits that we discussed already?

A. I probably have formed opinions based on the readings that I've done since then.

Q. Can you tell me what those opinions are?

A. I can in -- I can in response to specific questions. I don't come prepared with a list of all things that I've thought of since I've -- while reading those other documents.

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Q. Well Professor Rubin, I don't know what's in your head, obviously, so I can't ask you if X is in your head if there is unlimited number of Xs.
A. That's right. That's my problem as well.
Q. So I'm asking is there anything that you can recall going through your memory of opinions you have come to as an expert in this case that aren't expressed in these four documents.
A. I can try to give an example.
Q. You can give me as many examples as you can and if you say it can't be exhaustive, I understand that.
A. It certainly cannot be exhaustive.
Q. But as many examples as you can possibly give me.
A. It might be helpful if I had a list of documents I was asked to read for this.
Q. Sure.
A. Then I can -- that might help refresh my memory. One thing, I was asked to read some documents regarding imputations in NMES that were done when the Zeger team got the data set.
(Interruption by the reporter.)
A. The Zeger team did their imputations and I expressed some opinions, and I think in my first

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report, about imputations that were done including ones done by Zeger, but I've read some documents since then, for example, that indicate that people, I think the article was by Sommers --

Q. Right.

A. -- that certainly he was -- indicated concern with the excessive amount of nonresponse in NMES within the agency, and so I wasn't as aware of what the agency's concerns were before reading that.

Q. I understand you would like to see a list of the documents we designated for your deposition. That would help.

A. That would help to at least get going on some kinds of comments, and so --

MR. LOVE: Would you mark this as the next exhibit, then.

(Plaintiffs' Deposition Exhibit 3547 was marked for identification.)

BY MR. LOVE:

Q. Before I show you that list of documents that we designated for your deposition, let me ask you, Professor Rubin, had you reached other expert opinions in this case besides what are expressed in the four documents we have discussed already, up to and including the time that you submitted your

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supplemental expert report, Exhibit 3546?
A. Let me be sure I understand the question. Have I -- do I -- had I formed other opinions prior to January 10 that are not expressed in these reports?
Q. Right, January 10, 1998.
A. I certainly believed the major opinions that I had formed prior to January 10 are represented in these reports.
Q. By "major" do you mean any opinions of significance?
A. Of most significance. But I believe if -- Let me start that sentence again.
When I read through my prior deposition in this case, there certainly were lots of questions asked and I provided answers. I think those answers expanded upon major points in these reports. My memory is that the deposition went for a couple days so there are obviously things raised in the deposition and opinions offered in the deposition that aren't strictly represented here.
Q. Let's exclude opinions expressed in the four reports or during your previous deposition. Are there any other expert opinions that you have formed as of January 10, 1998?
A. There may be, there may have been some that were

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not asked about in the deposition by Mr. Hamlin but I certainly couldn't -- couldn't recover them now.

Q. I will show you what we have marked as Plaintiffs' Exhibit 3547, the supplemental pre-designation of documents for your deposition today, Professor Rubin.

A. Okay.

Q. Let me go through those one at a time and see if that helps you recall any opinions that you have reached that aren't expressed in the documents, four exhibits we discussed earlier or your previous deposition.

A. The first two are supplemental -- sorry. Let me correct that. The first one is my supplementary report. The next two are supplemental reports of Brian McCall and William Wecker, which I had not seen before, and I did read through them so I have opinions about those documents and the issues they raise. The next one, two, three, four I believe are orders from the courts which I glanced through to get a feeling for what the underlying legal decisions had been regarding what's -- what's allowable, although obviously, not being a lawyer, I read those with a lay eye, not an expert eye.

The next one is a supplemental report of Zeger,

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Wyant and Miller which I had seen before. The next listing is all demonstrative exhibits, which I had not seen.

Q. All right. And I believe Mr. Biersteker told me there was one demonstrative exhibit -- you may not know it by that name -- that you were involved in preparing and we can discuss that later.

A. That's the colorful one?

Q. Yes.

A. That I was involved in producing, and so actually -- and that is an example of opinions that I would have now regarding the missing-data issues that I would not have been able -- I would not have had with the same, hopeful, clarity I have now because it's so involved and intricate, and there is a summary now that I can refer to.

The surgeon general's reports I did not look at. I may have looked at a page or two. The Evolving Role of Statistical Assessments as Evidence in the Courts, that's a book and I did look at the pages that were apparently referred to at trial.

"Heart and Stroke Facts," I glanced at that.

Sommers' study, that I did, I did look at that. I may have seen that -- There are a couple Sommers articles and I don't have them delineated well enough

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to remember on the imputation that was done in NMES by the agency. I did look at Milliman & Robertson, which is a --

Q. Actuarial firm?

A. Actuarial firm. Thank you.

I did glance through the settlement agreement but again from a lay eye.

I did quickly read through number 16, the Miller, Zhang, Novotny, Rice & Max report.

I did glance at the Nurses Health Study, I'm aware of the Nurses Health Study as a separate study so I have a general understanding of what that study is.

I guess in the interim also, in the context of reviewing reports for similar litigation in other states, I have read other documents that haven't been filed in this state but have been related to other states which have -- which are attempting to do the same kinds of analyses, though not the same way necessarily, and so there is -- there are opinions that I have about that work and about the -- what they are trying to learn, what they are trying to do in those analyses that I believe are relevant.

That's a broad brush of the documents that I've looked at and since January 10 I think that probably

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includes most of them. This last comment regarding other cases, there are lots of documents in those other cases but I would have opinions about those documents, I have opinions about issues raised in those documents, and some of those opinions would at least have bearing on my opinions on what's being done here.

Q. You mentioned that you read documents in other state cases other than the Minnesota case?

A. Correct.

Q. Which -- You have some opinions having reviewed those documents that might apply to the Minnesota case as well; correct?

A. Correct.

Q. Please tell me about those opinions.

A. The -- Let me just ask Peter, this is -- there is no problem talking about this, Peter?

MR. BIERSTEKER: No, you can go ahead and start, although obviously there is going to be a certain level of detail beyond which -- we haven't made designations in all cases yet so in some ways this is consulting as opposed to testifying, although I anticipate Professor Rubin will be eventually a testifying expert. So we will let you talk about some of this stuff, but I don't want to get too far

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into it.

THE WITNESS: So you can't look out the window.

MR. BIERSTEKER: I won't look out the window.

A. The two cases I've been looking at most recently are Oklahoma and Washington State, and in Washington State the primary document that I've been looking at, there have been a lot of supporting documents as well, is the report by -- reports by Jeffrey Harris, and that's -- that report is of interest because he explicitly tries to address the misconduct issue and explicitly tries to estimate how behavior and medical expenses would have been different had the alleged misconduct not occurred, alleged misconduct by the tobacco industry.

Related to that, there were a collection of documents that he referred to including a book by Manning dot dot Newhouse dot dot dot that I had never looked at before, and I have glanced at that, read parts of that as well, and there are other documents in addition that Jeffrey Harris referred to that are -- that I do not believe were designated out of that -- I don't know the technical meaning, I don't mean it that way, but they didn't arise. This is my

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reading of documents for Minnesota. In fact, there is one that comes to mind now, there is a new report by I think it was Miller in November of '97 -- I think that's the date -- that I hadn't seen before, in which, for example, he refers to the missing-data problem in a different way than he did earlier.

Q. This is by Leonard Miller or Vincent Miller?

A. I'm blocking.

MR. BIERSTEKER: Actually, I expect if it's a state other than Minnesota it would be Vincent, but was it published or a litigation-type document?

THE WITNESS: I remember the date of November of '97 and I do not believe it's published.

MR. BIERSTEKER: It's probably Vincent.

A. There are other documents like that as well that Harris referred to that I was asked to look over. In the Oklahoma case, the main --

Q. Let me just --

A. I'm sorry.

Q. Stop with the Washington case for a minute.

A. Sure.

Q. Are there any opinions that you formed in connection with your review of these documents in the Washington case that you believe apply to the Minnesota case?

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A. Yes.
Q. And what are those?
A. Well the primary one has to do with the feasibility of attempting to do an analysis that addresses the alleged misconduct and the changes in behavior and healthcare costs in time that may have resulted from that alleged misconduct. Harris directly addresses it. Not necessarily the way I would do it, I'm not saying that. I think there are problems there. But he does address that issue in a serious way, so that indicates that -- I think it can be addressed. Again I'm not endorsing everything he did because I think there are serious problems there, but it is a -- it is a serious first-pass attempt to address the question in -- that has longitudinal aspects in time, addresses misconduct and attempts to address the issue of the effect of misconduct on both behavior and healthcare costs. Also the fact that in the other report by Miller, I guess it's Vince, that he -- he was a little bit more aware of problems with missing data than he was before, so he refers, for example, to my '87 textbook with Rod Little, and there exists more sophisticated methods to handle missing data, although he doesn't attempt to use it, but at least he is up one notch from where he was

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before in awareness.

Q. When he says, refers to, his '87 textbook or your --

A. My '87 textbook.

Q. Rubin and Little?

A. Yeah, Little and Rubin.

Q. Little and Rubin.

A. Right.

Q. What about the discussion of missing data do you -- led you to form any opinions that applied in the Minnesota case other than those expressed in the four reports you previously looked or your deposition in this case?

A. I'm not sure I understood that question.

Q. Sure. What about your review of this discussion of missing data in the Washington case that led you to form opinions that apply to the Minnesota case other than those you expressed in the previous reports that we discussed and at your previous deposition here in the Minnesota case?

A. Well previously it appeared to me that the methods of handling missing data were extremely naive relative to the literature and work that's been done in the past two decades on handling missing data problems in general and specifically in government

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survey type data sets. The fact that Miller referred to this book, which I think still is regarded as being the standard reference on missing data even though it's more than a decade old, indicates to me that -- excuse me -- that there was some awareness that there is a literature that's out there but they weren't really -- he wasn't, not "they," he wasn't really prepared to try to capitalize on that literature because it was buried in a paragraph that said yes, there are more sophisticated methods, or sentence to that effect, advanced methods or other methods, but, his assessment, they weren't worth doing here. So the reference to the book was just the reference to the whole book, not even a chapter or a section, so it was the kind of thing, well, gee, I guess I wasn't -- my reading might have been I guess I didn't know that there was -- that there was this literature, I should probably refer to it, but I guess I don't want -- it's too hard to do the work so I'll go back and do what I did before. That's an opinion about what they -- what the level of understanding of how to deal with missing data was in dealing with the NMES data set. That's kind of a bungled sentence but I'm trying to find something -- I'm trying to be very clear on what effect it has on

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my opinions about Minnesota.

Q. Right.

A. And I know I was --

Q. That's what I'm after, what effect does it have on your opinion in the Minnesota case?

A. That's a fair question. I realize I was sort of drifting into another direction, that's why I said "bungled sentence."

Q. Let me ask it so it's clear.

A. Yeah.

Q. What effect did reviewing this, some further discussion by an expert named Miller in the Washington case, have in terms of giving you any new opinions about the missing-data issue in the Minnesota case?

A. None with respect to what actually was done but I guess what I'm saying, the way they referred to the book, the way Miller referred to the book made me reinforce the idea when they handled -- when they, meaning the people handling missing data in all these -- in NMES missing data in all the states, my feeling before was they just weren't aware of the issue of the literature on this very important issue and this -- this quotation in the Miller book reinforced that feeling that they weren't aware of it

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at all, and it's oh well, we'll just go forward, so it just reinforced that feeling, that's all.

Q. If you look at your supplemental report, Exhibit 3546, and turn to page 8.

A. Yes.

Q. Is that where you began your discussion of what you call the causal question?

A. Correct. I believe that's right, yeah. That certainly -- that's what the section is titled there, although I do have some discussion of that earlier. This is a -- this is the supplemental report.

Q. Right.

A. The propensity score methods, just to be sure, the propensity score method, which are section I, are related to the causal question, too, in the sense that if you want to address causal questions and observation studies, you do have to worry about adjusting for collection of background variables, something called confounders, whose distributions differ in the exposed and unexposed groups, in this case smokers and nonsmokers, and these propensity score methods are designed to help that process of understanding whether you can do that adjustment realistically and how to do that adjustment through background variables, these confounders. So, section

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I is about methods that are closely related to causal questions. The section II, called Data Sets for Causal Modeling, are about data sets that maybe have something to say about -- about the topic of behavioral changes and healthcare changes in time after the date of alleged misconduct. So I want to be clear that section I, although it's entitled Propensity Score Methods, it's related to causal questions as well.

Q. Is section I relevant to estimating smoking-attributable healthcare expenditures leaving aside the causal question relating to defendants' alleged misconduct?

A. Yes.

Q. So it relates to both types --

A. Correct.

Q. -- type calculations or estimations?

A. Correct.

Q. Other than issues relating to what you call the causal question that we just discussed, illustrated to some extent in section II of your supplemental report, and other than what you just told me about the missing-data discussion in the Washington case, is there anything else about your review of documents in the Washington case that led you to new opinions

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about the Minnesota case?

A. Let me think for a minute.

Certainly the primary one would be this temporal analysis involving the date of alleged misconduct and what would have happened otherwise. There may be other things but they certainly are not as dominant as that one.

Q. My question is: Can you recall any other opinions that you believe, new opinions that would apply to the Minnesota case other than that causal opinion you just told me about?

A. And the very minor point about the missing data.

Q. Right.

A. From Washington.

The reason I'm pausing is I'm trying to think, when I glanced through, read parts of the Manning dot dot dot Newhouse dot dot dot book, whether there were things there. There were these issues of -- of -- it's a very economically oriented book about costs and discounting and I don't believe they really would change my opinions of anything in this case that I've talked about but it's possible if I thought harder about that something might arise, but nothing else comes to mind right now.

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Q. We now turn to the Oklahoma case.
A. Okay.
Q. What new opinions that aren't expressed in the various reports you previously submitted here in the Minnesota case have you arrived at based on your work and review of documents in the Oklahoma case?
MR. BIERSTEKER: Insofar as those opinions relate to Minnesota; right?
MR. LOVE: Yes.
MR. BIERSTEKER: Yes.
A. I haven't spent as much time looking at documents for Oklahoma as I have for Washington, and the Ok -- the main report there is by a fellow named Harrison. I don't remember his first name. He is an economist in South Carolina. And that analysis, I believe this is correct, does not attempt to do the misconduct, did not try to address the misconduct issue, and it's styled more like the Minnesota analysis starting with NMES and doing the adjustments for the background variables.
One thing it does do that may have an effect on my opinions in Minnesota is my memory is that Harrison attempts to adjust for a larger collection of background variables than do the Zeger team. My memory is it's something on the order of -- I don't

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know why this number comes to mind, but 38 background variables that I believe he regards as important to adjust for and in a later section of his report I think he does a bunch of tests of various kinds, I'm not saying I endorse these tests, but that he does tests to check the validity of his model and indicates that he believes that the model is a good model and these factors are needed, they are needed in order to get the right kinds of predictions. I've not carefully considered that, I haven't looked at the data at all, I haven't had -- looked at the files to see what I think of these variables, but the fact he is using a larger set of variables I believe and claiming that it's important to include all these variables might have an effect on my opinion in this case and the fact that a smaller set of variables is in fact used.

Q. Are there any particular variables that you believe weren't used in the Minnesota work by professors -- Drs. Zeger, Wyatt and Miller that should have been used?

A. I'm trying to remember. I don't have a list of the Harrison report in mind. It may have included variables like depression or other sorts of behavioral variables. I wish now I had looked over

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that before coming here today and I did not.
Q. Do you have an expert opinion that depression is a variable that should have been used in the Minnesota analysis?
A. I do not have an expert opinion on which -- which variables should have been -- should have been used in the analysis the way it was done. That caveat is important to me.
Q. What do you mean by "in the way in which it was done"?
A. Well there is an attempt to describe smoking -- sorry. There is an attempt to estimate this quantity called smoking-attributable fraction or smoking-attributable expenditures as a function of certain collection of background characteristics and the way these analyses are set up, this thing called smoking-attributable fraction or smoking-attributable expenditure is a description, what I call descriptive estimate, something you are trying to estimate that describes a population within certain cells defined by these background characteristics, and it's an attempt to compare like with like, as the phrase goes, to compare smokers and nonsmokers who are alike with respect to these background characteristics, and therefore it is a descriptive quantity to be

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estimated in the population with these cells defined by these background characteristics, and the list of background characteristics to be used in getting a smoking-attributable expenditure or smoking-attributable fraction that lists, then defines what this descriptive estimate is supposed to be, so that's one segment, that's a descriptive estimate.

Now in order to think of that descriptive estimate as having any causal interest, any causal interpretation either in the sense of misconduct or in the sense of if there were never any smoking, smoking did not exist, requires a whole collection of other assumptions to be brought on top of it. So that's the sense in which I'm saying that the -- that the list of those background variables to control for each list that you make up, there is a descriptive quantity and to -- let me stop there. I'll let you ask a question. Sorry.

Q. So to -- if you are trying to estimate smoking-attributable expenditures in this descriptive way that you have described, you just described, --

A. Right.

Q. -- and you don't deal with alleged misconduct of the tobacco defendants and you don't look at a world

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in which no one ever smoked but instead you compare smokers with similar never smokers, I take it you don't have any expert opinion of what variable should be used in that calculation?

A. I'm not sure which calculation -- you mean the descriptive, descriptive calculation. The -- Each descriptive calculation is another description. We can just compare smokers and nonsmokers or you can compare smokers and nonsmokers who are male or you can compare smokers and nonsmokers who are male and white or compare smokers and nonsmokers who are male, white and 34 to 65 and who are depressed and who drive recklessly, and to -- so all those things are descriptive quantities and if the -- if the target of estimation is a descriptive quantity like that, then given that it's a descriptive quantity, my job as a statistician is to say, okay, how do I try to estimate that quantity. Well given the data that I have at hand at this point, there are some descriptive quantities that are being set up to estimate the control for more or less of these background characteristics, so my job as a statistician is to say how do you estimate those quantities well, and the Harrison report in Oklahoma, for example, his analysis adjusts for a bigger

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collection of background characteristics and so that would say that there are more precise in the sense of more precise for each person descriptive demands that some people think should be estimated than were estimated in Minnesota.

Q. My question is whether you, Professor Rubin, have an expert opinion as to whether more of those background variables should have been used in the Minnesota calculation to do what it purports to do.

A. Should have in the sense of getting more precise comparisons in the sense of more -- more conditional, more conditioning on more things and therefore more precisely relevant to a particular person or particular class of people, the bigger the collection the better. In a descriptive sense, it's more precise, so that's -- so that's one answer.

Q. That's one answer. But if you are trying to estimate the total cost spent, let's say, by the state of Minnesota through its Medicaid program during a certain time period, in a descriptive way that Dr. Zeger, Wyant and Miller did, do you have an expert opinion that there are other background variables they should have used that they didn't use?

A. Well the descriptive question is a descriptive question and you get to decide which background you

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wish to condition on, so the opinion there would be if you want more precise comparisons in the sense of being more specific to an individual person of certain characteristics, then the bigger the set the better.

Q. My question is: If you are not trying to estimate for a particular people but you want to know if the state of Minnesota's Medicaid program, for instance, what the total expenditure was over a 18- or 19-year period, do you believe they should have had more background variables that they considered in their analysis?

A. Yes, if the objective is to compare like with like, which is what I keep saying. I keep reading, not I keep saying. I apologize. I keep reading in the descriptions they want to compare like with like and in the standard epidemiological approach, that's what's to be the objective. And even if just total costs at the end are desired totaling over the whole state, if you want to make those comparisons comparing smokers and nonsmokers who are as similar as possible with respect to the background characteristics, the phrase like with like, then you should include as many as possible to do that comparison.

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Q. Would you agree that the final result will change less and less as the additional factors or variables are taken into consideration, have less to do with the relationship between smoking and healthcare expenditures?

MR. BIERSTEKER: Just a moment. I'm going to object to the form. I'm not sure I understand the question but if you understand it, you can answer, Professor.

A. I think you stated a tautology which if something is irrelevant, then when you adjust for it it will be irrelevant, but you may have meant if it appears to be, based on some analysis, unrelated, does that mean that properly adjusting for it would have no effect, and that's wrong. In other words, one of the problems I see in the analyses that have been done is the adjustments that are being done are not being done in a reliable way and if things appear to be relatively unrelated they may in fact not be, and so if the adjustments aren't being done correctly, they may -- doing it correctly may have a big effect.

Q. Professor Rubin, in your opinion, to do the best practical job in estimating the smoking-attributable expenditures in the ways that Drs. Zeger, Wyant and

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Miller did, what additional background variables would you suggest be taken into consideration be used?

A. Well again the -- in the descriptive sense, if you want to compare like smokers and nonsmokers, you should have as long a list as you can, and the list that's available certainly includes more variables than had been included in the Zeger et al analysis. Moreover, the way one then does the comparison of the smoker-nonsmoker, conditionally given this list of background characteristics, has to be done more carefully, I believe, than was done by the Zeger team.

Q. Is it your opinion that they should have used every variable that was available in NMES?

MR. BIERSTEKER: I'll object. That's asked and answered.

A. I believe I have answered that before in the sense that the more variables that you include the more specific an answer you get, specific to a particular type of person, and therefore the more you have a descriptive answer that is satisfying this desire to compare like smoker and like nonsmoker where like is defined by this collection of background variables.

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Q. If you were given the task to estimate what Dr. Zeger, Wyatt and Miller called smoking-attributable expenditure in the state of Minnesota's Medicaid program, what additional background variables would you put into your model?

A. Well I would probably want to discuss the issue with epidemiologists, perhaps medical people, perhaps economists to find out at what level of specificity they wanted this descriptive S demand to be, and then I would work with them towards getting good estimates of this -- of this quantity, smoking-attributable fraction or smoking-attributable expenditure at that level of specificity.

Q. Have you had such conversations with other experts, health economists and so on?

A. I'm thinking. It's possible. I don't think I've had specific conversations about that topic, no, but I certainly have read a variety of these reports that address the issue of what those background characteristics should be and that's the context in which the Harrison report, because it has a longer list, arose. So that somebody he talked to who is an economist, perhaps by himself or people he talked to in the Oklahoma case thought that the list should include more variables than were included in

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Minnesota.

Q. Based on what you have read so far and what you know so far, if you were given the task today, are there any additional variables that you would take into consideration?

A. So could you set the stage a little bit better? It's a hypothetical situation.

Q. Sure.

A. I'm being retained by the state of Minnesota to redo the Zeger analysis, is that it?

Q. You are being retained to estimate smoking-attributable expenditures the way Zeger, Wyant and Miller do for the Medicaid program in the state of Minnesota over a period of time, several years, and my question is: Based on what you have reviewed today and so forth, are there any other variables that you would put in your model that they did not use in their model?

MR. BIERSTEKER: I'll object to form.

A. Well I guess I would say I would look around at other analyses that people had done like the Harrison one. Maybe the Harrison one, although I guess he refers to documents done by other people, and would look to see the collection of variables that other people think are worthwhile adjusting for, and then I

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would say, okay, let's try to adjust for the biggest collection we can, which includes everything that someone might think is important, and if someone says no, I want to back off that, like back off using depression and get this descriptive smoking-attributable expenditure, smoking-attributable fraction, not adjusting for depression, then that's easy to do having already done it for depression, it's easy to go the other way from an intellectual sense, at least. What I would do then, I would include at the beginning an attempt to adjust for any of the background variables that anyone ever thought were relevant to see what would happen and then when someone says, okay, I want to leave that one out, then we could back off and leave that one out. Having done the harder task first, it's easier to back off, so I guess I would adjust for anything. I would do it in a model at least where I would get those quantities at a more precise level, more specific level than they did because other people think it's worthwhile doing. And just as in the -- as in the final sort of analyses that are done, there are many more variables in NMES that are used to get the smoking-attributable, refined analysis at least, that are used to get smoking-attributable

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expenditures, smoking-attributable fractions than are used in the summary tables where they just do it by age and sex, for example.

So they do it -- they -- they essentially do the same thing that I'm saying I would do; that is, they adjust for a bigger collection and they report at a more accumulated level, more summary level, and I'm saying let's do the analysis, then, and if other people think you should adjust for background variables, let's try to adjust with a full set and then accumulate the results, collapse the results if desired.

Q. And sitting here today, based on whatever conversations you have had with other people, whatever, you have been able to read from other experts in other fields, are there any particular variables you can tell me you would include in your model?

A. I think I've answered that, that I would try to include all the variables that anyone thinks are important.

Q. I'm asking --

A. Specifically.

Q. -- identify those variables for me.

A. Okay. Let's see. I think depression is one

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that people had mentioned, maybe alcohol consumption is another one that people have mentioned. I think there are other indicators of risk-taking behavior that people have mentioned that lead to other kinds of medical costs. I haven't looked at the Harrison report for weeks so I wish I had it in front of me, which I do not.

Q. That's what you can recall today?

A. Pardon?

Q. What's what you can recall today?

A. That's what I recall today at this moment. During the day, lunch, other things may arise, and so -- but sitting here right at this moment, those are the ones that pop to mind.

Q. If other things arise, please let me know.

A. Sure, I'll make a note of that. I suspect they will because it's the kind of thing you can't recall it when asked the -- can't recall when asked the question directly, but they are in the back of your mind so they pop up.

Q. Other than adjusting for other background variables, is there any other opinions that you now have in the Minnesota case that you arrived at while reviewing information in working on the Oklahoma case?

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A. There was an issue in the Oklahoma case -- I wish I could remember this again more clearly -- of -- on what population or subpopulation the estimation was done, and there was an issue of how you dealt with the Medicaid population. I'll try to get to that, and I -- I'm a bit fuzzy about it now, but I believe it was different than was done in Minnesota and in order to recover that I'd have to either think harder or go find the Harrison report. But I believe there was an issue of how it dealt with the subpopulation of public-aid recipients from NMES that was done differently than the way it's done in Minnesota. I didn't review that before today because it's -- I've wanted to keep clear on what was being done here and keep the other stuff out of my mind but perhaps I should have glanced through that.

Q. I just want to know what opinion you have about the Minnesota case now.

A. Yeah, I understand that.

Q. If you --

A. But you asked the question based on what I read in Oklahoma.

Q. Right. And so I mean do you have a different opinion about the Minnesota case having looked at this issue maybe of some populations in NMES?

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A. It's -- it's possible because I think the estimation was done, the estimation in NMES was done not only using more variables but I think with a different way of dealing with the public aid Medicaid subpopulations in NMES. I just don't have it clearly in mind. I apologize.

Q. I take it you have don't have a clear opinion on the Minnesota case in mind on that issue right now.

A. I guess the clear opinion would be there is an issue to be thought about and perhaps the analysis that was done in Minnesota, since my memory is that it differs from the analysis that was done my Harrison for Oklahoma, perhaps the Minnesota analysis is less good, less relevant than the Oklahoma analysis. In order to decide that, I have to become much clearer about the way the Oklahoma analysis was done and if in fact it does deal with that subpopulation issue differently, which I believe it does.

Q. Anything else, any other opinions about the Minnesota case that you arrived at as a result of your work and review of information in the Oklahoma case?

A. Well one goes back to the missing-data problem. I don't know if this really affects my opinion to

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handle what was done in handling missing data in Minnesota, but Harrison tries to avoid any imputation at all because he states, I believe, somewhere in the report that it's subject to dispute and he doesn't want to get into the kinds of problems, he views I guess as problems, that, for example, arise in Minnesota. He does some recoding of variables which he thinks gets around the missing-data problem and then discards subjects that have missing data after he has done this arbitrary coding, and so-called complete case analysis, although it really isn't. It has problems.

His analysis has problems but in some ways they are different problems from the way they are handling missing data in Minnesota. Does that affect my opinion of what was done in Minnesota? It doesn't affect my opinion of what was done. I've always had the opinions that have been written down there on the way they handled missing data which I'm quite critical of, but it -- the report from Harrison suggests that other people are now aware of the problems as well since he is trying to avoid getting in the same predicament.

Q. Looking back at the pre-designation of documents for this deposition, Professor Rubin, Exhibit 3547,

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item number 2 was the supplemental expert report of Professor McCall.

A. Right.

Q. I believe you told me you have reviewed that now.

A. Yes, I have.

Q. Have you reviewed Professor McCall's original expert report?

A. No, I have not.

Q. All right.

A. It was -- But I did review the supplement and so I don't have a good tie between certain parts of the supplement and the points made in the original report.

Q. I understand. When did you first review Professor McCall's supplemental report?

A. I believe that was on Saturday, just this past weekend.

Q. How much time did you spend reviewing it?

A. I -- Saturday and yesterday, maybe an hour in total, although it's a short document. The way it's tied, it's hard to make contact for me for some reason, so I probably spent about an hour.

Q. Item number 3 is the supplemental report of William E. Wecker dated January 15, 1998.

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A. Right.
Q. You saw that as well?
A. Yes.
Q. Did you see Dr. Wecker's original expert report?
A. No, I did not.
Q. You have never seen that?
A. I've never seen it.
Q. When did you first see Dr. Wecker's supplemental report?
A. Same time as the Brian McCall, Saturday.
(Interruption by the reporter.)
Q. And Sunday, too, I take it?
A. Yes.
Q. This past weekend?
A. Correct.
Q. How much time did you spend reviewing Dr. Wecker's supplemental report?
A. Probably about the same amount of time. In some sense, it might be more time, it might be less, depending on how you count time. What I mean by that is, both documents are very short but then the time spent looking at that and trying to look at other materials that tied into that and thinking about how it relates to other things, going back and looking, for example, at the supplemental report of the Zeger

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team, the original report of Zeger team, so it's probably more time total in -- where that was the stimulus to think about things. That sentence makes sense, sort of.

MR. LOVE: I'll mark this next.

(Plaintiffs' Deposition Exhibit 3548 was marked for identification.)

BY MR. LOVE:

Q. Professor Rubin, I'll show you what we have marked as Exhibit 3548 to your deposition. It's two -- one figure but takes up two pages, and then three pages of footnotes that go with it; is that correct?

A. That is correct. There is a whole collection of footnotes going up to whatever letter it is, U.

Q. From A through U.

A. Yeah, yes.

Q. And I understand from Mr. Biersteker that this is the only demonstrative exhibit in the Minnesota case that you were involved with.

A. Correct.

MR. LOVE: Is that correct, Peter?

MR. BIERSTEKER: I suppose -- I believe that's correct. Let me say this: The simulation that was the electronic media attached to Professor Rubin's first report does generate graphical displays

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that I suppose you could count as a demonstrative, although I'm not sure they were designated as such. Okay? That's my own -- that's my only hesitation.

MR. LOVE: To the best of your recollection, then, the ones you demonstrated as demonstrative is the only one he was involved with.

MR. BIERSTEKER: That's correct.

MR. LOVE: What we have just marked as 3548?

MR. BIERSTEKER: I believe that's correct.

Q. With that understanding, Professor Rubin, I'm not going to show you hundreds of demonstrative exhibits and ask you if you were involved in them but I will talk to you about this one that you were involved in.

A. Okay, fine.

Q. First of all, let me confirm you were involved in preparing this exhibit, 3548?

A. Absolutely.

Q. And the two-page figure, is it meant to be laid side by side or how is it used?

A. Yes. The first one goes to the left and the second one goes to the right, so you will notice that they are lined up so the data sources, which is the first column labled data sources, each is the data

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source that was used in some of the Zeger et al analyses and so they run across the row, so NMES with the supplement runs across. That's the first row. The second row is the NMES without supplement, goes all the way across. Each column, that's after the data source column, which is the title, refers to some subset of variables that are being used in some analyses somewhere by the Zeger team, so each column refers to a block of variables, or maybe only one variable. Like the second column is labeled race, that refers to the variable race, which is available in all the data sets.

Q. Okay.

A. And whereas something like smoking status, which is also of -- a variable, actually a small collection of variables, is available in NMES but not in the supplement. It is available in BRFSS but not available in billing records, et cetera.

Q. Okay. I realize that the top of this Figure 1 describes to some extent what the chart does but I want to make sure I have it straight in my mind. If a box in this chart is white, what does that mean?

A. That means that there are data available there in that -- the data source, which is the column -- look at age and gender to be clear.

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Q. Sure.

A. The first upper left corner, that's the NMES with the supplement and look at age and gender, they are almost always fully observed. That's why it's blank. Then the next one is race and that's blank, that means it's almost always fully observed. That's why it's blank.

Q. The next one says "Smoking Status" and there is a "5% or 9%" listed above that.

A. Right. I believe footnote A explains that, I hope it does. It has to do with the way they do the coding. Footnote explains that and says, "The percentage of missing values depends upon the type of analysis," is one of the things awkward about the analysis they are doing, the analyses tends to be moving targets. "It is 5% when smoking status is coded as Ever smoker vs Never smoker and it is 9% when smoking status is coded as Current, Former or Never smoker. Subjects with missing smoking data are treated as former smokers by the plaintiffs," so that's one of these what I would regard as an arbitrary imputation.

Q. All right. Then the next column in that same first row is blue for "Physician Diagnosis of Medical Condition." What does that mean?

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A. That means do we have a variable that describes from the physician the diagnosis of the person's medical condition, and that's not in NMES. We don't have that. We have self-report, which is two columns over.

Q. And blue means there is no data at all?

A. There is no data, right, there is in physician. Where they did NMES, they didn't say get a report from your doctor, Xerox it and mail it in and we will code it in NMES. They didn't do that. Nor did they get a recording of the amount reimbursed to the provider. But we have a physician diagnosis as a medical condition, not in NMES or in BRFSS but in the billing and enrollment data, Medicaid and Blue Cross/Blue Shield data we actually have a physician diagnosis of the medical condition, and that's why those squares are blank, indicating that they -- there are real data there, may have errors in them but at least there appear to be real data there.

Q. And then the -- There is only three, let's say, colors of boxes on this chart, correct, white or blank, blue and green?

A. Correct.

Q. And what does green mean?

A. Green means the data are not used in any

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analyses that we could figure out, at least, although they were available in the database.

Q. Does it mean that the data is the same in the blue characteristic where it's almost entirely available?

A. Blue means it's almost entirely missing, it's gone.

Q. I'm sorry, the same as the white, meaning it's almost entirely variable?

A. Correct, almost entirely available. In some cases it shows a footnote that shows it's missing. For example, in BRFSS, self-report of physician communication and medical conditions, there is a green box there. It's not used. And I believe the 95 percent means 95 percent missing, so that's -- may be the reason why it wasn't used in that particular -- in the BRFSS analysis, although it was used in NMES. Am I being clear?

Q. Look to footnote F in that particular box if you wanted to know an explanation --

A. Correct.

Q. -- why it wasn't used there?

A. Correct, what is going on there.

Q. Professor Rubin, what is the significance of this exhibit?

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A. Well the significance can be described in several ways. The first way, which is just as important to me having done these kinds of things for a long time, is that there is a -- there is a big compound data set. What I mean by "compound," it draws data from different sources and it uses all the data to try to reach some conclusion, and since all these databases are being used for some piece of the analysis, ideally there would be no missing data, there would be no blues, no percentages, everything would be complete and then an analysis would be done of the sort that the perhaps Zeger would do, there are analyses -- The Zeger team would have done an analysis based on that complete data. I think everybody agrees, I believe, that wouldn't it be nice if we had, for example, in BRFSS and the billing records all the -- in the Minnesota billing records all the information on everybody, on everybody in the state, for example, so instead we don't have that. We have missing data various places and so the kinds of analyses that have to be done try to piece together in some synthetic way -- I think that's the jargon in statistics and what's used in the Zeger report to describe it as well -- to get synthetic estimates, because we don't have all this

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information.
Q. And typically in applied statistics, anyway, if a problem is reasonably complex, you often have the situation where you don't have data sets that have all the information that you would like to have in one data set and there is no missing valves in any of those?
A. Correct.
Q. Data.
A. That's right. It's -- Let me separate that, though. It's extremely common to have some missing values in the data set you are dealing with, that's extremely common.
Q. That's where if it's a survey, for instance, some respondent did not answer every question?
A. Correct. So for example if we look at NMES and look at the columns self-reported physician, it's missing 3 percent of the time. If you look at total medical expenditure from provider, which is dollars, it's missing 53 percent of the time, so that's NMES. That's not uncommon. In NHANES, it's a survey I'm more familiar, there is unit nonresponse and item nonresponse. That's very common. It's less common that an analysis will require putting together many data sets from different sources to reach a

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conclusion, that's less common, and when you do that, it is true that you often have these blocks of blue because one data set will have some kind of information that's not available in another set. So I just want to be clear there are two kinds of answers because what is very common is missing data due to unit nonresponse or item nonresponse, putting data sets together to form one giant data set to form an analysis is less common but when you do that it's quite common there would be blocks of missing data.

Q. Beyond what you just told me, is there anything you are trying to illustrate to the jury with this exhibit?

A. Well to illustrate that the massive amount of missing data here, the massive amount of blue, which means that synthetic estimation based on assumptions, where the assumptions come from outside the data themselves, have to be made in order to draw the kinds of conclusions in order to do the kinds of analyses that you want to do. Because there is so much missing data, that you have to do something else. That's one point. That's the blue boxes.

Q. Let me ask you about that.

A. Okay. There is another point as well having to do with the white boxes.

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Q. Let's ask about the blue boxes first of all. Isn't it true that -- I'll withdraw that.

Professor Rubin, are you aware of any data sets that do not contain blue boxes that could have been used in this analysis?

MR. BIERSTEKER: Object to the form.

A. I'm not aware of any existing data sets but the existence of these -- of these blue boxes for the Minnesota-specific data suggests that perhaps some data should have been collected, especially in an issue of this importance, to get real data.

Q. And I understand that it's always possible to go out and try to create new data sets but my question really was focused on existing data sets. You were right. As I understand it, you are not aware of any existing data sets that would have all the information that's shown in the various columns in Exhibit 3548 and had you put that data set on this chart would not have blue boxes showing missing data, entirely missing data?

A. Correct, correct. It does emphasize the desirability if this issue is regarded as important to try to collect data, gather data, assemble data from existing records that doesn't have all that blue.

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Q. Have you explored the concept of gathering additional data or creating additional data sets for the Minnesota situation?
A. Explored in the sense of being active and making specific suggestions beyond saying look at BRFSS for Minnesota, billing records for Minnesota, wouldn't it be nice to do a survey of those people and collect background information on this kind of information that's in fact missing? No, I haven't, I haven't done anything beyond that general, broad statement. There is nothing more than saying wouldn't it be nice to make an attempt to fill in these blue boxes from specific data that's relevant to those rows, those data sources.
Q. Right. And with NMES, for instance, that was done in 1987; correct?
A. NMES was done in 1987; correct.
Q. Do you believe it would be possible to go back and find those people and fill in the blue boxes?
A. Well that wouldn't be necessary if the Minnesota-specific data had that information. My understanding of these analyses is that the reason why NMES was -- was used is because it had this detailed information, the white boxes there going across, so a model could be built and then

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extrapolated to or grafted onto the Minnesota data that was -- that was wholly deficient in that way.

Q. Well let's look at the billing data shown on here --

A. Correct.

Q. -- Exhibit 3548.

A. Okay.

Q. You understand that that billing data goes back as far as 1978, perhaps, and certainly back into the early 1980s?

A. I understand it goes back a ways, yes.

Q. Is it -- Do you believe we can go back and find all those people and fill in all the missing values?

A. No, I'm not claiming that would be possible, to find all those people, or even a simple random sample of those people, a random sample that would be easily accessible, but presumably as you become closer in time to the present, it becomes easier and easier to do that. As you get towards the late '80s, '87, '88 '89, '90, '91, '92, so forth, it becomes easier to get data on those people and that would have been nice to have all those blue boxes filled in with random samples of people from those years themselves.

Q. When you say it's easier to get data, you mean it's easier today to go back and find those people

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and get the data from them now?
A. Presumably it's easier. I'm not an expert at all on what these databases look like, what the billing records look like or what BRFSS looks like.
Q. Let me ask you --
A. But --
Q. -- right there, professor, have you ever looked at the claims data in this case?
A. No, I have not.
Q. Have you ever looked at the BRFSS data in this case?
A. No, I have not, but I was going to say that it's not really necessary to get the exact same people. If you want to get relationships, you can get relationships, billing records in Minnesota to these background characteristics from people that are more current if you wanted to do that and then apply those relationships that you see, say from people in 1993, and see how Minnesota Medicaid recipients, Blue Cross/Blue Shield recipients, how they -- how their expenditures and billing records relate to their -- these other variables that are available in NMES. And one could argue that generalizing from Minnesotans in 1992, and let's say they were done in 1992, to Minnesotans going back in time, and these

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were all Medicaid, Blue Cross/Blue Shield recipients, its a better idea, less extrapolation, than taking a national NMES database for 1987 and extrapolating that to all Minnesotans going back in time, back in the '70s, which is what the current process is.

Q. Do you have an expert opinion as to whether it's practical to go back and get enough data from 1993 and subsequent years from the claims data to do what you just suggest?

MR. BIERSTEKER: Object to the form.

A. I've been involved in things like -- in surveys like that, helping to design surveys like that. If you have billing records and you have in recent times, mid '90s, and you had people's names and addresses and you wanted to collect NMES-type information on them, you could design a sample and you could draw them and you could find out, you could try to get them in, get the information, get the NMES-type information. I don't know. Presumably the billing records have names and addresses and maybe more information but it's not -- certainly that's what survey people do all the time, but I have no specific knowledge about these billing records.

Q. Other than the various calculations that are found in your original expert report and your

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supplemental report and these two other exhibits that -- from your previous deposition, Exhibit 3544 and 3545, and any calculations we just looked at in Exhibit 3548, are there any other calculations that you have done in connection with the Minnesota case?

A. Any calculations that I directed, let's say?

Q. Sure.

A. Okay.

Q. Done yourself or directed others to do.

A. I don't believe so. Specifically for Minnesota.

Q. Right. Are there any calculations that you believe apply to Minnesota other than what we found in these exhibits that we just identified?

A. The only reason I'm hesitating is, there were some similar propensity score calculations that were done, I believe for Texas, using a different set of background variables because it was relevant to the Texas case, and I don't remember whether in fact we used a different set of background variables or the same, but it's a similar kind of analysis, also from NMES, getting to the same point. But I think my memory is that in fact we may have actually used the Minnesota variables for that because that was what was available at that time.

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Q. Wouldn't change your opinion about propensity scores --
A. No.
Q. -- how they apply to the Minnesota case?
A. It would not.
MR. LOVE: Take a break?
MR. BIERSTEKER: That's fine.
(Recess taken from 11:02 to 11:12 a.m.)
BY MR. LOVE:
Q. Professor Rubin, have you performed any analysis for the Minnesota case that's not described in your original expert report, your supplemental expert report, the two Exhibit 33 -- I'm sorry -- 3544 and 3545 and this last exhibit we just talked about, 3548?
A. No, I've not.
Q. Have you worked on any charts or graphs or tables or diagrams not shown in those documents we just described for the Minnesota case?
A. I don't believe so. Let me -- There may have been a question asked of me about something someone used as input to such a thing but I've not seen a chart. I have -- I have some notes that I made up but they are not -- they are not displayed, just to remind me what all the analyses are and stuff, but

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that's -- those aren't analyses and statistical analyses, as I think you are talking about, analyzing the data.

Q. They are notes to remind you about things?

A. Of what all the analyses that they did are in outline form.

Q. May I see that, please?

A. Yes. I have a copy here.

Q. Great, thank you.

A. I'm not sure it's right.

Q. That's what you have?

A. Yeah, it's what I've put together. I think this is another copy. I have two copies. They are putting together with conversations with -- with my data person.

Q. Who is your data person?

A. This is Raghunathan, R-A-G-H-U-N-A-T-H-A-N.

Q. The same person who --

A. Correct.

Q. -- did the calculations we saw on Exhibits 3544 be and 3545?

A. Correct, same person.

MR. LOVE: Let's make this as the next exhibit.

(Plaintiffs' Deposition Exhibit 3549 was

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marked for identification.)

BY MR. LOVE:

Q. Professor Rubin, I'll show you what we have marked as Plaintiffs' Exhibit 3549.

A. Correct.

Q. Is that a copy of the notes you just handed to me?

A. Yes, it is.

Q. Okay. And you have a second copy of that there.

A. Yes, I do.

Q. So if we talk about that, you can use your copy and I can use the marked copy.

A. Fine.

Q. Can you tell me just generally what this exhibit is?

A. It was an attempt to understand and summarize the collection of analyses that are done in the Zeger report. The reason for doing this is, the Zeger report as written is very thin on describing what is actually done in many cases and so it was an attempt to get a better overview feeling for -- for what is done and how it changes by type of analysis they are trying to do.

Q. Did you have an opportunity to review the computer data that was submitted along the Zeger

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reports, the original and supplemental?
A. I did not myself but Raghunathan did and so this is the result of many back and forths with him trying to figure out what was actually done in the analyses.
Q. And you are relying on him for the actual calculations that are behind the Zeger reports?
MR. BIERSTEKER: Object to the form.
A. Calculations in the sense of reading the code to figure out what calculations they were doing rather than -- it's not two plus three kind of calculations, it's calculating what they were -- are they doing A plus B here or doing C plus Z here. So I haven't -- I'll stop there.
Q. Is there any other work that you performed in connection with the Minnesota case since writing your supplemental report dated January 10, 1988 that you haven't told me about already this morning?
A. Work that I did, that would include reviewing documents, I reread depositions, I obviously talked to Peter Biersteker. That's work but did it produce a document or an analysis? No, that I haven't told you about.
Q. No further documents or analysis?
A. I don't believe so.
Q. Did you review the transcript of the deposition

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of Dr. Wyant that was taken in January of this year? I think it was January 26th.

A. Yes, I did, shortly after it became available, so it's not -- I glanced at parts of it yesterday and the day before, but I did review it, yes.

Q. I take it you did review the supplemental report of Drs. Zeger, Wyatt and Miller dated --

A. Yes, I did.

Q. -- November 17, 1997?

A. Yes.

(Plaintiffs' Deposition Exhibit 3550 was marked for identification.)

BY MR. LOVE:

Q. Dr. Rubin, I'll show you what we have marked as Plaintiffs' Exhibit 3550, titled Smoking Attributable Health Care Expenditures: Blue Cross, Blue Shield of Minnesota and State of Minnesota, 1978-1996, Supplemental Expert Report of: Drs. Zeger, Wyant and Miller, November 17, 1997.

A. Okay.

Q. Is that the supplemental report of the plaintiffs' experts that you reviewed in this case?

A. It looks to be the same one that I read before. Yes, I think it is.

Q. And the data that came with that report you gave

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to Mr. Raghunathan?
A. Raghunathan, yes.
Q. Raghunathan, and he analyzed that for you?
A. Analyzed in the sense of did whatever analysis was done, yes.
Q. Have you reviewed any of the transcripts from the trial in this case?
A. Yes, I have.
Q. What have you reviewed?
A. I reviewed Zeger, I reviewed Wyant, I may have glanced at part of Samet. There may have been pieces of other ones I looked at as well but not with the care that I looked at Zeger and Wyant.
Q. When did you review the Zeger transcript, trial transcript?
A. I think shortly after they became available, so like -- I could try to look at a calendar to try to figure that out better but I -- my guess would be the end of January.
Q. If I told you they actually testified at trial the last few days of February --
A. Of February.
Q. -- of 1998.
A. Okay, then it would have to be -- then it would have to be within a week or two after that.

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Q. Okay. And the same is true for Dr. Wyant.
A. Correct.
Q. He testified shortly after Dr. Zeger.
A. Right. I remember that. So it would be -- In fact, perhaps it was even days after they testified.
Q. Did you review --
A. It was days after they testified, yes, within a day, yeah. It was very fast.
Q. So it would have been early March?
A. Yeah.
Q. Did you review them again in preparing for your deposition today?
A. I did not really review them. I think I glanced through them to -- I did look. They were open. But "reviewed" suggests I actually started at the beginning again and read the whole thing and that I did not do.
Q. Are there any particular parts of those transcripts that you did look at more -- in more detail to prepare for your deposition today?
A. I'm trying to remember. I think there was one part of Wyant where I read one page. I don't even remember what the topic was right now.
Q. Okay. If you look back at Exhibit 34 -- I'm sorry -- 3546, your supplemental report.

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A. Yes.
Q. Again in particular looking at topic Roman numeral I, Propensity Score Methods.
A. Correct.
Q. With respect to that section of your supplemental report, did you rely in any way on the supplemental report of Drs. Zeger, Wyant and Miller, Exhibit 3550?
A. I don't believe any more than I relied on the first report.
Q. That was really my question, Professor Rubin, is whether the analysis and discussion here in section I of your supplemental report is really based on what was set forth in the original report and nothing different than was set forth in the supplemental report.
A. I think, yeah, that's basically true. Perhaps the only relevant change to my analyses that I report on in this section would be the fact they don't to this testimation, bring variables in, knocking them out, as much as just bringing everything in.
Q. What they call the full model in the supplemental report?
A. Full model; correct.
Q. And when you did your propensity score analysis

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set forth in your supplemental report, did it matter whether Drs. Zeger, Wyant and Miller used testimation or full model approach?

A. No, because the issue is, I have these group -- this group of smokers and nonsmokers within the cells and how much they differ on this collection of background variables that you are using to describe like, what "like" means, and therefore you want to adjust for differences in those background variables, and whichever way they tried to adjust, whether the original or the supplemental way, comes under the same criticism that's -- criticism that's revealed by the analyses that I do there.

Q. And does your propensity score methods section of your supplemental report and the analysis you did there depend in any way on the relative errors that are set forth on page 6 of the supplemental report of Zeger, Wyant and Miller, Exhibit 3550?

MR. BIERSTEKER: I'll object to the form.

MR. LOVE: I'm sorry, the wrong -- I think that's the right page.

A. Not in any direct way. In an indirect way -- Let me make sure I'm answering the question.

Q. Let me ask a different question.

A. Fine.

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Q. Because I think I understand something you were trying to convey.

Other than the fact that those relative errors were based in part upon NMES data --

A. Uh-huh.

Q. -- is there any other way in which the relative errors reported on page 6 of the -- of Exhibit 3550 affected your discussion in section I of your supplemental report, propensity score methods?

A. No, not in the way they affected the analysis of the report there. However, the -- if you couple those very large relative errors with the results there that are in the propensity score method section, it has perhaps more impact on what you can draw from the -- conclusions you can draw from the analyses that they did.

Q. Did you describe in your supplemental expert report any impact of comparing the propensity score analysis with the relative errors reported in the Zeger report?

A. No, I did not; no, I did not.

Q. Did you make -- Did you give any consideration to looking at the relative errors in the Zeger report at the time you wrote your supplemental report?

MR. BIERSTEKER: Object to the form.

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A. Well in a general sort of way, yes. Just as part of doing statistics when you are trying to adjust for background variables, one of the issues that has to be in the back of your mind is how well relationships are estimated, relationships with the background variables to smoking and nonsmoking in relationship to these background variables to outcomes such as healthcare costs. So, those -- those relationships, if they are purely estimated, are -- have a general kind of effect on how important the problem -- what the impact of the problem would be, but they didn't affect the analyses that I'm doing here, which is solely focused on how much the background variables differ, the distribution of them differ between smokers and nonsmokers within these cells.

Q. Referring to the analyses you are doing here, you mean in section I of your supplemental report?

A. Correct.

Q. And based on the chronology that we have been discussing today, I take it that you did not rely on the expert reports of any of the other defense experts in this case, in particular Dr. Wecker or Dr. McCall, when you wrote your supplemental report?

A. Correct, I haven't seen any.

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Q. Did you rely in any way on any information provided by Dr. Samet when you provided your supplemental expert report?
A. I don't believe so. Do you mean provided by Samet when I read testimony? This is other than -- than is in --
Q. Either in Dr. Samet's reports or testimony or deposition or anywhere --
A. No, I don't believe so. Well except to the extent that Samet is an epidemiologist and was aware of the need to adjust for confounding variables, so that's what this analysis is about, how well can you adjust for this collection of confounding variables that apparently that team has agreed should be adjusted for, and Samet was involved in, presumably to some extent at least, maybe limited, in deciding what variables, what background variables should be in there to adjust for, and so this analysis is saying can you adjust for that set that that team agreed on should be adjusted for and how well can you do that, and that's what these analyses, propensity score analyses address. They address that question. So in that very general sense, I guess I'm relying on something that Samet said.
Is that helpful or just rambling? I don't mean

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it to be rambling.
Q. I know.
Was there anything specific that Dr. Samet said that you are relying on?
A. No.
Q. Did you consult with anyone other than perhaps Dr. Raghunathan in determining whether to conduct propensity score analyses in this case?
A. No, I did not.
Q. Did you consult with anyone other than him in performing a propensity score analysis in this case?
A. No, I did not. Well only in the general sense. Again that I talked to Peter Biersteker about why I thought this would be a relevant thing to do because what the game is is comparing like with like and background variables have different distributions in the two groups. You have to be very careful about how you do that, and that's what these techniques, propensity score techniques are designed to do.
Q. But other than that, you didn't consult with anyone else?
A. Correct.
Q. Now what data sets did you use in this propensity score analysis set forth in section I of your supplemental expert report?

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A. This is a data set that the Zeger group provided to us on NMES and it was a data set with their imputations, even the ones I regard as completely wrong, had just accepted it because I didn't want to get into that issue, said okay, let's take that data set that they used and they are using to make these adjustments, what is -- what's the evidence that those adjustments are being well done or poorly done, accepting the data that they created.

Q. Did you perform a propensity score analysis on any data sets from the behavioral risk factor surveillance system data sets in this case?

(Interruption by the reporter.)

A. No, I did not.

Q. Did you perform any propensity score analysis on data sets from the National Health and Nutrition Examination Survey, NHANES?

A. No, I did not.

Q. Did you perform any propensity score analysis on the Minnesota -- the state of Minnesota's claims data for it's Medicaid program or its GMAC program?

A. No, I did not, but I should probably clarify at this point that except for possibly NHANES, the propensity score analyses would have been irrelevant in BRFSS and would have been irrelevant in the

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billing, Minnesota billing records, because that's not where the SAFs and SAEs were estimated adjusting for background characteristics.

Q. I'm just asking --

A. Okay.

Q. I want to make sure I understand what your propensity score analysis was performed on because it's not completely clear to me from the language in the report --

A. I understand.

Q. -- just what you did.

A. I apologize.

Q. I take it there were no other data sets other than the NMES data set provided to you by Drs. Zeger, Wyant and Miller on which you did perform a propensity score analysis in the Minnesota case?

A. Correct.

Q. Now you told me that you used -- Let me back up a second.

The NMES -- You are familiar with the NMES data set itself, that NMES --

A. In a general sort of way.

Q. -- makes available?

A. Yes.

Q. Have you ever examined that data set?

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A. Have I personally ever examined the data set?
Q. Yes.
A. No, only through other people doing things for me. I've not held that data set in my lap.
Q. Have you ever asked one of your colleagues to examine that data set for you?
A. Other than Raghunathan?
Q. Including him.
A. Well these analyses are based on that data set so I must have asked him to do that.
Q. Well I thought you said the analysis was based on a NMES data set provided to you by Drs. Zeger, Wyant and Miller along with whatever imputations were in the data at that time.
A. I'm sorry, ask the question again.
Q. Sure. I'm trying to distinguish between that data set that may have some imputations in it as opposed to the NMES data set that if you went to NMES itself and said please can I have your data from the 1987 survey?
A. Correct. I think the only data set we had available was that provided by the Zeger team.
Q. Okay.
A. But I can't be positive about that, but I think that's the only one I had.

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Q. Do you think that's the data set that actually is analyzed in your supplemental report?
A. Yes.
Q. Now does that data set have -- Withdraw that.
The NMES survey itself, as you've indicated in Exhibit 3548, had some missing data in it; correct?
A. Before -- At what point? Yes, it has missing data, absolutely.
Q. And it has missing data of a nonresponse type where a survey person did not provide answers to all the questions that they were asked; is that correct?
A. Let me try to clarify.
Q. Sure.
A. There are generally two kinds of missing data not described, one is called "unit," one is called "item." A person who provides much information and leaves some items out, that's call "item," and somebody who doesn't do anything is called "unit" non-responded, and I suspect you are saying are there missing data problems of both kinds.
Q. That is what I'm saying, Professor Rubin.
A. I'm trying to be helpful.
Q. Thank you.
A. And the NMES, when you think of it as including the supplement, the supplement can be viewed as a

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unit nonresponse because people did not -- a group of people did not respond to that, but throughout all of NMES there is item nonresponse on particular kinds of things, in particular the medical expenditure questions.

Q. Did NMES itself or the agency for health care policy that conducts NMES, make any type of imputation for that type of missing data, the unit nonresponse and the item nonresponse?

A. For the missing expenditure data, they did. It may be for some other things as well but the primary one that I was concerned with was the expenditure data. I'd have to scan this display and look it at all the footnotes to make sure I'm not messing up on what other items they may have imputed. I think there was but I don't remember which one it was that the agency imputed before releasing it.

Q. But certainly before releasing the data to anyone including Dr. Zeger, Wyant and Miller, NMES made it's own imputations for some missing --

A. Correct.

Q. -- values?

A. Correct.

Q. And did the data set that you examined in your propensity score analysis described in your

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supplementary report have those imputations in it?
A. Yes, presumably, because that was the data set that the Zeger team used and the Zeger team just accepted those imputations. Again I accepted the imputations as well as the Zeger group imputations just to disentangle the issues can you adjust for these variables as they had them in a reliable way from the issue of missing data.
Q. Is it your testimony that in addition to whatever imputations that NMES may have made to its data set, the date set you examined also had additional imputations by Drs. Zeger, Wyant and Miller?
A. Correct.
Q. What additional imputations were those?
A. Well I can just look at them. Some imputations were on smoking status, I believe seat belt use, I believe overweight. Is that from them? I'd have to take time to look through these. I could give you -- One reason for doing this is so I didn't have to try to fill my mind with things that are details, so subjects of missing smoking data treated by former smokers by plaintiffs, that's an imputation. Self-report of medical conditions, that's not missing very much. C is done by the agency. H is poverty

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level. What was done there? It was done by the agency, that was done by the agency. Kind of health insurance, I believe that was done by the Zeger team. Yeah, I believe that's right. Marital status I believe was done by the Zeger team, education I'm almost sure was done by the Zeger team, I believe seat belt use was imputed by the Zeger team. It's on the next page. Check that. Yeah. The way it's described here isn't quite right. But overweight status I believe is done by the Zeger team, treated as a different category, self-report health status. So, certainly there were those variables that were imputed by -- by the Zeger team in addition to the ones that were handled by NMES people or the agency.

Q. Can you draw any conclusions about the validity of using the NMES data supplied by the agency with only it's own imputations for doing the work Zeger, Wyant and Miller did based on your propensity score analysis of the data set that has not only NMES's own imputations but also additional imputations by Drs. Zeger, Wyant and Miller?

MR. BIERSTEKER: Object to the form.

A. I'm not sure I understand the question. Maybe I can restate it, what I think you are saying.

Q. Sure, if you want to.

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A. Do my propensity score analyses reveal anything about the effect of the imputations that were done for missing data by either the Zeger team or the agency? No, that's not? That's not it.
Q. I'll try again.
A. Sorry.
Q. The propensity score analysis you did including imputations made by the agency and imputations made by Drs. Zeger, Wyant and Miller; correct?
A. Correct.
Q. Does that propensity score analysis tell you anything about using the NMES data set as provided by NMES before anybody makes imputations to perform the kind of -- to use in the kind of model that Drs. Zeger, Wyant and Miller developed apart from their imputations?
A. I'm not sure I follow the question.
Q. Were you trying to estimate smoking-attributable health expenditures in this descriptive way you told me about?
A. Correct.
Q. Do you know from your propensity score analysis of the data set you were provided whether the NMES data set that the agency provides could be used for that purpose in a linear regression fashion?

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MR. BIERSTEKER: Object to the form.
A. I'm still not exactly sure. I guess what I -- I'll try to answer it and see if this is. The analyses that I did, the propensity score analyses I did in this section, suggests that the data as provided by the agency and as filled in with missing data by the Zeger team does not support the use of the kinds of linear regression or log-linear regression methods that the Zeger team used to do the adjustments, it does not support the validity of those adjustments.
Q. That's the conclusion you draw from your propensity score analysis?
A. Correct. That the groups, the smokers and nonsmokers even within these age/sex cells, differ markedly on the collection of background variables they want to adjust for and differ to an extent that previous work says that you cannot trust the kinds of modeling adjustments that they are making, "they" meaning the Zeger team were making in their analysis.
Q. My question, Professor Rubin, is: Does your propensity score analysis lead you to any conclusions about whether you could use the NMES data set as provided by the agency without any additional imputations by the Zeger team to perform the kind of

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appropriate regressions and other linear regressions that they performed?

A. I think I know what you are driving at. The problem that's revealed by the propensity score analyses is not a problem with the way the Zeger team imputed data. It's just a state of nature that the people who were smokers and nonsmokers, as reported by NMES, differ substantially in background characteristics and therefore you cannot do the kind of adjustment for those background characteristics in the relative naive way that they are doing it and get reliable answers. It won't work.

Q. So --

A. That problem is not created by the imputation that the Zeger team is doing. The problem is created by the state of nature. Smokers and nonsmokers differ relatively substantially with respect to the background variables that they say they want to control for.

Q. So it's your testimony that the data set as supplied by the agency that conducted NMES could not be used to perform the kinds of linear regressions that the Zeger team did in their work?

A. Well "can" is a funny word. They did it so it can be used. When the results come out should you

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believe them? No.
Q. Okay.
A. So it can't be used to get reliable results. But obviously you can do it in the sense I can jump out a window. It's not very good for my health but --
Q. But in your opinion, anyone who tried to use the NMES data set supplied by the agency to perform those types of linear regression analyses would produce results that you don't believe are reliable?
MR. BIERSTEKER: Object to form.
A. That's correct, for those kinds of analyses, for the purpose they were doing them, that's correct, I would not -- I would not believe in those results.
Q. Would your opinion be the same for someone trying to use the NMES information without even -- with no imputations, not even those made by the agency?
MR. BIERSTEKER: Object to form.
A. I haven't done the proper propensity score analysis leaving out imputations, but based on these analyses, I think the answer would be yes, that even if I did the proper propensity score analyses, accounting for the missing data, that I would still find that smokers and nonsmokers differ substantially

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with respect to these background characteristics. In other words, I don't believe the imputations that were created by the agency or by the Zeger team made the groups look far apart. I think they are -- I think smokers and nonsmokers are far apart with respect to the background variables they want to control for, and moreover, they may be far apart on patterns of missing data, which is something else that if you are going to do the analysis correctly you should try to adjust for.

Q. If you had the NMES data without any imputations by the agency or anyone else and performed what you believed to be proper multiple imputation on that data set to fill in the unit non-responses and the item non-responses, do you have an opinion as to whether a propensity score analysis performed on that data set would lead you to the same conclusion that you can't use that data set to perform the kinds of linear regressions that Drs. Zeger, Wyant and Miller do for the purpose that they perform them?

A. That's an excellent question. In fact, for the purpose of propensity score analyses, I would not use multiple imputations. I would use that later on to try to get estimate of the SAFs and smoking-attributable expenditures. But the correct

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propensity score analyses to determine whether the smokers and nonsmokers were far apart with respect to the background variables would actually leave the missing data as missing and also try to worry about whether the smokers and nonsmokers differed with respect to their patterns of nonresponse. There was a thesis on that a couple years ago for a student of mine, probably going through his last stages in -- through the publication process now, but he addresses that issue of what should be a proper propensity score analysis, proper adjustment when you have an exposed and unexposed groups that differ not only in values of background variables but also perhaps in the missing data patterns.

Q. So in order to determine whether a data set should -- can be used properly in your opinion to perform linear regressions, you should look at the propensity score analysis of the data set without any imputations; is that correct?

A. Without any imputations created, that's correct, because it may be that the patterns of nonresponse do differ between, in this case, smokers and nonsmokers, in which case comparing like with like doesn't just mean the same age, the same marital status but the same proclivity to have -- to not respond to

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something, not respond to the age question, not respond to some other question.

Q. Is it possible that the propensity score analysis on that unimputed or missing value data set could be either -- show better conditions for using linear regression or poorer conditions using linear regression than a propensity score analysis on the same data set after imputation?

A. Good question. Almost certainly, I think I said this before but to be clear, almost certainly it would be worse.

Q. Worse before the imputation or after the imputation?

A. It would be -- Yeah. I should say that the groups would almost certainly be more different doing the propensity score analysis the correct way leaving out imputations because not only can the groups differ on the values of variables that are reported but moreover they could differ on the patterns of nonresponse. That's another reason they can be different. There are more ways which can be different using the correct analysis.

The way imputations are done, they are probably making the smokers and nonsmokers look more similar and not allowing for the fact that they -- that they

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might have missing data patterns that are different. I don't know whether they have missing data patterns that are different. It's like adding another variable to adjust for in there and if it turns out the missing data patterns are the same for smokers and nonsmokers, then you are back where you were with this analysis, basically. But if the patterns differ, then you ever a bigger difference to adjust for, so they are farther apart than they were before.

Q. Is there any possibility that the propensity score analysis of the data set with the imputations would produce results that suggested that it was more favorable for linear regression than the propensity score analysis for unimputed data set?

A. I'm saying its likely. These analyses are reported, are making it more favorable for regression than the -- a more proper analysis would be. So my feeling is that the situation is worse than this, and that's why I didn't want to get into doing -- I wanted to just accept what they did for imputations and do that analysis to show that even there, even doing -- even accepting their imputations, that doing the analysis the way they are doing it to get SAFs is not reliable.

Q. Is it possible that performing the propensity

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score analysis on the imputed data set would show conditions less favorable for linear regression analysis than the propensity score on the unimputed data set?

A. I don't believe so. I think I sort of said that. It's almost --

Q. I understood you to say that you would think probably doing the imputation would make the situation appear more favorable for doing a linear regression.

A. Correct.

Q. My question is, although that's your belief what's probably going to happen, is it possible it could happen just the other way around?

A. Let me think for a second because that has to do with how the imputations were not done well and so here you are doing something incorrectly and doing one thing incorrectly could it make it worse. In fact the way these imputations were done, as I'm scanning through the things that were done, I think they would only tend to make the smokers and nonsmokers look more similar on the background variables that are being imputed than they -- than they really are and therefore I think doing it the right way would only -- would only reveal a bigger

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difference.
Q. So it's your belief the way the imputations were done in this particular situation would make the propensity scores on the imputed data set appear more favorable to linear regression than otherwise?
A. Correct, correct. I could try to clarify because I think in almost all the imputations, for example, the smoking variable is never used to do the imputation. Smoking itself is imputed but I don't think the other analyses use smoking to do the imputation, which means that when you are imputing for smokers and nonsmokers, using the same relationship and making smokers and nonsmokers look closer together than they would be if you did it correctly. So that's the -- the current basis of that opinion based on just thinking about it now with you. I haven't gone into deep thought before about it, although the general issue is obviously I have given it lots of thought over the years. But in response to this question, I'm almost certain that that would be true.
Q. As a general matter, not on these specific data sets but just in general, is it possible at least that imputations could make the situation under propensity score analysis seem less favorable for

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linear regressions?

A. If you did it in a -- really incorrectly. I'll give you an example of what I have in mind. Let's suppose that we are missing -- what's a variable that's missing a fair bit that's used in these things? Overweight status, seat belt use, marital status, let's suppose we imputed all the smokers to be married and all the nonsmokers to be divorced. That would make it look bigger on marital status because they would look more different on marital status than they were before, presumably at least if we picked the modal category for smokers and imputed marital status, let's say most smokers are married, we made all smokers married and most nonsmokers are not married, all the nonsmokers not married would exacerbate the difference and make it look bigger. You could do something and make it bigger but that's a hypothetical question that wasn't done here at all. I can make up examples like that to be -- you know, they are kind of silly, though.

Q. I understand. I just want to find out, it can happen that way?

A. Yeah.

Q. It depends on what the imputations did?

A. Not in a situation like this where they are

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doing imputations the way they are doing it.
Q. Have you looked at each of the imputations that the agency itself did to determine whether they made smokers and nonsmokers more alike or less alike?
A. I think they made them more alike because I don't think that there is sequential hot deck imputation method used smoking as one of the factors so therefore automatically they are making it more similar than they would be otherwise. I think that's correct.
Q. Do you think that the imputations that Dr. Zeger, Wyant and Miller did to the NMES data set made smokers and nonsmokers more alike or less alike?
A. I believe they made them more alike, again for the same reason. I don't believe they used smoking as a variable when creating the imputations for the other variables, so therefore they are using some average value. I think that's correct. I could look at these charts again and read through the footnotes to try to refresh my memory but that's my -- that's my global memory now, is that they were done that way.
Q. But your testimony is that is how imputations are done without taking smoking into consideration, then that will make smokers and nonsmokers more alike

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than before the imputations?
A. Correct. Because it's treating them the same way.
Q. Professor Rubin, did you prepare any drafts of your supplemental expert report, Exhibit 3546?
A. I'm sure I did.
Q. Okay.
A. I almost never write the thing and let it be done.
Q. Did you discuss those drafts with anyone as you were doing them?
A. Yes.
Q. With whom?
A. I discussed them primarily with Raghunathan and I discussed them with Peter Biersteker, too.
Q. Did you provide copies of those drafts to anyone?
A. Yes.
Q. To whom?
A. I provided them to Raghunathan and Peter Biersteker.
Q. Do you currently have copies of those drafts?
A. No. I get rid of -- Just generally when I write stuff, there are so many drafts that are created, I work on many papers at the same time. If I kept

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drafts around I would be totally confused, more than I am now.

Q. You did create drafts on the way to the final report; correct?

A. Yes, but as soon as I had a new one I threw away the old one.

Q. But you did give copies to Raghunathan and Mr. Biersteker?

A. Yes. Not as many --

Q. Not all of them?

A. -- versions as I went through but I thought here's something that looks pretty good, I think, what do you think? Do you understand what I'm saying? Is it clear, the point's clear? If not, I'll have to clarify.

Q. Were they given hard copies or disks or how -- what form did the copies take?

A. I think most of it was fax back and forth.

Q. Hard-copy fax?

A. Hard-copy fax, I think.

Q. Do you know whether Raghunathan saved any of his copies of the drafts?

A. I would hope not because he was my former student. He would get nothing done if he kept copies of all this stuff, but I don't know.

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Q. You don't know one way or the other?
A. I don't know one way or the other, no.
Q. Did you make any attempt to provide plaintiffs with your drafts of the supplemental expert report?
A. No. I don't think the drafts -- This is an aside. I don't think the drafts changed except the labeling of columns to make it clear what was going on and writing them, to my memory.
Q. Did you have any work papers that you --
Besides drafts, did you have work papers that you used that you were writing your supplemental report and doing the analysis on which it was based?
MR. BIERSTEKER: Object to form.
A. I'm not sure what a work paper is.
Q. Any other paper besides a draft of the report, whether it's just a piece of paper on which you performed some analysis or whether it's commuter output or whatever it might be.
A. No, I don't believe so. But just to clarify, there certainly were articles that are referred to in there that I had available but that's --
Q. I'm referring to the work of you or your colleagues --
A. Oh.
Q. -- on this case --

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A. No.
Q. -- as opposed to previously --
A. No, no.
Q. -- existing articles.
A. No.
Q. So nothing like that was created during your work on the supplemental report?
A. No. At least not that I remember. I don't think so.
Q. Earlier you were discussing documents that you reviewed either in preparing for your deposition or in -- just in your general work on this case; correct?
A. Correct.
Q. And as one guide, we were using the supplemental pre-designation Exhibit 3547.
A. Correct. Well that number, I'll accept the number. I assume it's right.
Q. Item 11 on that pre-designation Exhibit 3547 is the book titled The Evolving Role of Statistical Assessments as Evidence in the Courts; correct?
A. Correct.
Q. And you told me you had looked at that?
A. I had looked at that and in particular the section that seemed to be relevant to an issue that

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arose in the trial.
Q. When was the first time you looked at that book?
A. Saturday.
THE WITNESS: I'll have to tell Steve Fienberg, my friend, you are Xeroxing instead of buying it.
MR. LOVE: Off the record a second.
(Discussion off the record.)
BY MR. LOVE:
Q. Doctor Rubin, I'll show you what has been marked as trial Exhibit 26,046, the evolving -- titled The Evolving Role of Statistical Assessments as Evidence in the Courts, and ask if that's a copy of the book that you reviewed on Saturday.
A. It probably is. It feels about the same number of pages as the Xeroxed copy that I have in my possession as of Saturday.
Q. So you were viewing a Xerox copy --
A. Correct.
Q. -- rather than a printed book itself?
A. Correct.
Q. Fair enough. Now I think you mentioned that -- well do you know the editor of that book, Steven Fienberg?
A. Yes, I do.

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Q. How do you know him?
A. I've known him probably since 1968. He was a graduate student in the department of statistics a couple years ahead of me, I guess, at Harvard. We bump into each other relatively frequently.
Q. And as I understand this book, if you look at the second or third page in, it's the result of work of what's called the Panel on Statistical Assessments as Evidence in the Courts.
A. By Panel on Statistical Assessments as Evidence in the Courts, yes.
Q. And that it was sponsored by two committees, one being the Committee on National Statistics and the other being the Committee on Research on Law Enforcement and the Administration of Justice?
A. That's what it says, yes.
Q. Okay. And have you ever been a member of the Committee on National Statistics?
A. Yes, I was.
Q. And were you a member of that committee at any time when Stephen Fienberg was also a committee member?
A. I'm trying to remember. Steve was chair and Burt Singer became chair but Steve would still come to meetings, but I don't know if he was on the

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committee.
Q. He attended meetings of the committee but you don't know if he was an official member at the time?
A. Right.
Q. Because if you look just a few pages in where they list the Committee on National Statistics for 1986-1987, you will see Stephen Fienberg's name listed as at chair, as you indicated.
A. Right. So he was -- he was chair for a while and then Burton Singer, whose name I guess is not on it, came in afterwards, I believe, and I believe I was on the committee for a couple, three years when Burt was chair.
Q. Looking still at that same page at the members of the Committee on National Statistics, the third name is Seymour Geisser; is that right?
A. Yes.
Q. Do you know him?
A. Yes, I do.
Q. He is at the University of Minnesota?
A. Correct.
Q. And I notice there was a Nan Laird at the department of biostatistics at the Harvard School of Public Health?
A. Yes.

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Q. I assume that's a woman.
A. Correct.
Q. Do you know her?
A. Yes.
Q. And also in terms of Harvard representation, there is John W. Pratt at the graduate school of business?
A. Yes.
Q. Do you know him?
A. Yes. I think I know everybody here.
Q. Okay.
A. So you don't have to go one by one. The only one I don't know very well is Courtenay Slater.
Q. Do you have any understanding as to whether this -- the panel on statistical assessments as evidenced in the courts is a panel of the Committee on National Statistics?
A. Well I assume -- I think it does say that, doesn't it?
Q. That's my understanding but I wanted to know have you looked at it.
A. Right. The way this is set up, the Committee on National Statistics is really like a committee designed to form committees. It itself, at least in my experience, doesn't do much work in terms of a

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project but what it does is it spends time trying to decide what are projects that are worth doing, then try to round up funding for the projects and then trying to select members to be on the panel and the panel actually does the work in terms of writing a report for, typically, for the National Research Council, National Academy of Sciences, and if they think it's generally interesting they can look for an outside publisher for the report. So I was involved in one of these early on, I don't know when, on missing data. There was three volumes, committee on national panel for incomplete data, and the results were in three volumes that were published by, I don't know, maybe MIT press.

Q. So that was another panel of the Committee on National Statistics?

A. Correct.

Q. As you said, you know most of people listed as committee members of the Committee on National Statistics?

A. Correct.

Q. In this particular book.

A. Right.

Q. Are they all well-respected statisticians?

A. Well, Jerry Hausman is an economist; Tom Juster

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does some social statistician, survey work, very good understanding of that; Jane is a demographer; John is business school; Jim, ES; Courtenay Slater, I'm not exactly sure, sort of more of I think a policy-type statistician; Judy Singer is a sociologist with some strong interest in statistics, does some things in statistics, and Ken, Ken Wachter also has a demography side.

Q. Would you say that these people are all well-respected experts in their fields, anyway?

A. I think they are. I mean --

Q. All the ones that you know.

A. Yeah. Experts in their fields, not being an economist, not being a sociologist it's sort of unfair for me to say but I think they are. Certainly the point of getting people on the Committee of National Statistics is to try to get a good representation of people from diverse areas that have interest in statistics and are well respected, so I certainly believe that would be the reputation, statistics and in the fields in which they really reside.

Q. And for those people who are in the field of statistics, for instance Stephen Fienberg and Seymour Geisser, they are well respected in the field of

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statistics; correct?

A. Absolutely.

Q. If you look at a few pages earlier, you will see a listing of people on the Panel on Statistical Assessments as Evidence in the Courts. Do you have that page, Professor Rubin?

A. Uh-huh.

Q. In terms of the people on that who are in the field of statistics, like Stephen Fienberg and Paul --

A. Meier.

Q. -- Meier and Sandy --

A. Zabell.

Q. -- Zabell, are they all respected experts in the field of statistics?

A. Yes.

Q. Were you involved at all in --

A. So is Bill Hunter.

Q. And I didn't mean to leave anyone out. But the people who are in the field of statistics on the panel are well-respected experts in that field?

A. Yes, they are well-respected statisticians.

Q. Did you yourself have any involvement in this particular project?

A. No.

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Q. Based on your knowledge of the people involved in the project and the review of the book that you were able to make over the weekend, do you consider it a reliable authority in the field of using statistics as evidence in courts?

A. I don't know how to answer that because I'm not an expert on using statistics in courts. I'm sure that's what it was intended to be and probably may well be but the reason I'm hesitating is the way these panels work sometimes, they have very limited input from some people. Some people put a lot more time in it than others. No one gets paid anything. The people that are getting paid are the staff people and consultants, so the people at the bottom of the list there, so Miron Straf is a study director, and research associate and staff associates are people that are actually paid to do these things, consultants are paid. See, it's hard to know how much time different people put in. In some panels they have I've been on that have a list like this, one or two people do all the writing and the staff associates put it all together and in other cases people, everybody participates.

Q. Does the fact that Stephen Fienberg is the editor of this publication indicate that he had

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substantial involvement in it?

A. I would suspect he did.

Q. Okay. And I understand your -- you are a statistician and expert in statistics and not in the law.

A. Correct.

Q. And my question then is: Given your knowledge of the people, in particular Stephen Fienberg as the editor and your general knowledge of the workings of the Committee on National Statistics, any review of this particular book over the weekend, is it fair to say it's a reliable authority on the statistical discussions that it sets forth? Maybe you can't opine on the application of those statistical discussions for the law but as far as the statistical discussions themselves go?

A. It's probably very reliable with respect to what the current thinking is on that. I guess the reason I'm hesitating there, I haven't read it, I don't know whether I would agree with some of the statements and uses, but it's probably very reliable on what current thinking is and what the current state of these things are. I suspect it is very reliable.

Q. And if you look at any -- even a textbook on statistics that status -- professors of statistics

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would often use and would say is, you know, reasonably reliable, there may be parts or sections or even a single page or sentence in that book that you in particular or another statistician might disagree with; correct?

A. Correct.

Q. But you would still consider the work in general to be a reasonably reliable authority?

A. Yes.

Q. And the same thing is true for this book?

A. I assume so. I haven't read through it.

Q. The whole book.

A. But based on the people involved, many of these people, Paul Meier has been involved in statistics and law for many, many years. He is in his 70s. Yeah, Sandy's involved in these things. Yeah, I assume it is but I -- but I can't say more than that with respect to having read it or agreed with things.

Q. Sure. You said there were certain portions of the book that you tried to look at over the weekend. Would you identify that for me?

A. I may need some help but it was a section on confidence intervals and point estimates that was --

Q. I believe there is an index in the back if this helps you. I don't know if you used the index over

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the weekend to find things or --

MR. BIERSTEKER: I can probably help out.

MR. LOVE: That would be fine.

MR. BIERSTEKER: I think it's pages 114 to 115, approximately, which is the portion of the book about which Dr. Wyant testified in his testimony.

Q. Can you turn to those pages, Professor Rubin?

A. Okay. Okay, I'm on page 114 in the exhibit. Yes, that's what I -- I think I started on that and glanced through that part and a little bit further on.

Q. And that's --

MR. BIERSTEKER: John, I just noticed there is some highlighting on the copy you gave to the Professor.

MR. LOVE: Oh.

MR. BIERSTEKER: I don't know if that's inadvertent. I wanted to note for the record it was already there before we got it.

MR. LOVE: Sure. We can certainly provide a Xerox copy.

MR. BIERSTEKER: I don't care. I just wanted to note that it was there before it came.

MR. LOVE: That's fair.

BY MR. LOVE:

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Q. So starting on page 114, there is a section called "3.3.5 Damages"?
A. Right.
Q. So you reviewed that section?
A. I reviewed and thought about the paragraph at the bottom of page 114.
Q. Okay.
A. And I read quickly through the rest.
Q. The rest being --
A. Through the end of the chapter. It was just a quick read.
Q. Just tell me where you stopped reading.
A. One seventeen, I believe. Yes.
Q. Did you read the summary that's on page 117?
A. Quickly. If you ask questions about it, maybe I could read it begin.
Q. I just wanted to find out where you started and where you stopped.
A. That's basically what it was.
Q. All right. Did you come to any expert opinions about what's set forth in those pages?
MR. BIERSTEKER: Object to the form.
A. I don't know if they would be called expert because I am not an expert in interface of law and statistics. I thought about this issue of point

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estimates and the role confidence intervals play in damages, and whether I agreed with the -- with the drift of it, there are some things I don't -- I don't agree with that they were saying.

Q. Let me try to ask a more clear question, then.

A. Okay.

Q. Because you said you weren't an expert in the application of legal rules to these topics.

A. Correct.

Q. In reviewing these pages, did you come to any conclusions of your own regarding what's set forth there?

A. Conclusions of my own concerning --

Q. Just you said you reviewed this and did you come to any conclusions whether you agreed with parts or disagreed with parts or don't understand parts?

MR. BIERSTEKER: I'll object to the form.

A. Well there are some parts that I said -- there is a sentence I don't understand, don't agree with at all, the last sentence on 114, goes to 115. It seems to be an odd way of saying. I didn't understand that, but that's a minor point.

Q. Okay.

A. A more general issue is this idea of what role, I believe what role confidence intervals should play

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when they are large, when assessing damages, and kind of thinking of that, what was going on in my mind is, if the whole analysis is -- I can understand this if it's an individual situation, individual case, I guess, where you try to make projections into the future. There is always some uncertainty about what's going to happen. But you have sort of the full set of data in front of you that can be obtained or can be reasonably expected to be obtained because you don't have data in the future. Someone has been damaged, we don't know what his life history would be like in the future necessarily without it, so we have to make assessments of that kind. I can understand at that point the idea of confidence intervals relative to point estimates, that -- that confidence intervals may be relatively -- viewed as relatively unimportant, relative to the point estimates, although at the same time my thinking was there is another kind of uncertainty which has to do with model uncertainty. You don't know how to do the projection in time, that some people think of as being within confidence intervals that are not. Confidence intervals arise because of sample variability. This is focused on sample variability and if that's the best data you have, I can

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understand where that's true.

In another situation where the whole analysis is based on surveys, population level, where you could do a better job, for example taking this case where you could do a better job by going out and getting some information, for example from Minnesota people, and getting the right information to drive those confidence intervals down, I don't understand why this argument really should hold. If it's the best data you can get, then I understand needing a point estimate being adequate. The confidence interval will be what it will be. Why that should apply to a situation where you could get better data, you could get more focused data and there is huge amounts of money involved, I don't quite see why the fact that there is a gigantic confidence interval should say that's all you have to do. You don't have to do anymore despite the fact the confidence level is gigantic. Don't you have an obligation to collect better data because its available? So I sort of disagreed with that but that's in context. I don't know -- The title of the chapter is on -- what is it on? I don't know if it even applies to the case. That's my legal naivete. Maybe they are focused already on this antitrust stuff. That's the kind of

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thinking that took place when I went through that.
Q. Okay. If there were some reason why you could not go out and survey the Minnesota claims population, either a legal obstacle or any other obstacle that would just prevent that from happening, would that change your thinking as you just described to me?
A. To the extent it becomes -- you have done the best job possible, then I think these arguments seem to intellectually have more force to them. In this case, there is -- in the Minnesota situation, there is so much extrapolation taking place from existing data sets which appears to be -- potentially be unnecessary, at least without trying to get more relevant data, that I don't see it being in that state yet. But to the extent that it were true, that it's impossible to get better information to try to try to drive the confidence intervals down so we have some belief that what we are what we are estimating is fairly accurate, if it's impossible to do better, then that that carries weight. That carries weight also for the sensitivity, differ assumptions, different modeling assumptions. If you can't do a better job, then I guess you have to live with what you have got. But to the extent that it's an

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important issue, you should explore carefully to the extent possible that you can get better answers and do better analyses than just off-the-top-of-the-head analyses. Not off the top of the hat. Bad description.

(Interruption by the reporter.)

A. I was trying to adjust my answer to use a word I didn't want to use.

Q. You didn't mean "off the top of the hat"?

A. No. What I meant was, analyses that are not carefully done in the sense of being attuned to issues done by the propensity score analyses that I did, the fact that you can't rely on the answers that are being produced for reasons that are evident in the data themselves.

What I was thinking about when I said "off the top of the hat," just to be clear, was this statement somewhere in here that regression analyses are commonly done and are therefore the method of often of choice.

Q. When you say "somewhere in here," you mean somewhere --

A. In the book.

Q. Fienberg book?

A. Fienberg book.

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Q. Trial exhibit --
A. There is a sentence somewhere that says that -- I don't know where it is now -- that very often the regression analyses are used and that would -- not off the top of the hat, is a bad description for that. An off-the-shelf model without really thinking through what the consequences are. There are big damages involved, it would seem to me appropriate to think carefully about what an appropriate analysis model data combination, appropriate combination that addresses the real issue and not just use a regression analysis because it was -- it was used in this way one time before.
Q. Professor Rubin, I think we probably should break for lunch soon. Before we do, I want to, sort of helping me understand your propensity score analysis and truly from a layman's point of view, I was going to ask if we could after lunch, if you could sort of walk me through a simple example and I can tell you before lunch and maybe it helps you to think about it.
A. Sure.
Q. Maybe it doesn't.
Here's my simple example I'd like to try to walk through and understand how the propensity scores are

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calculated and how the comparison at least of the means between two variables are calculated and that your first test, your first test, I think, in your expert report, whether the means are a certain amount of standard deviation apart. And in my example, we just have two variables, smoker, yes or no, and gender, either male or female. I want to try to set up what I think is called a two-by-two table where the smoker variable is either zero if you are not a smoker or one if you are a smoker and the gender variables are the zero if you are a male or one if you are a female, and that in this population sample we have got 100 male nonsmokers, 200 male smokers, 200 female nonsmokers and 100 female smokers. Does that make sense to you?

A. Yes.

Q. So after lunch, if we could try to walk through that. If taking that helps you at all, you are more than welcome to take it.

A. Sure.

Q. Then let's break for lunch.

(Luncheon recess taken at approximately 12:27 p.m.)

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A F T E R N O O N S E S S I O N

(Deposition reconvened at approximately 1:39 p.m.)

BY MR. LOVE:

Q. Professor Rubin, I showed you this two-by-two table that I put together just before lunch and told you I'd like to have you walk me through this little example so I understand how propensity scores are calculated and how you go about making at least the first comparison, the comparison of the means of the two different variables, I guess you would call them.

A. Okay.

Q. Two different groups. So can you sort of do that and walk me through? If you need a calculator, I'll be happy to get it for you.

A. No, I don't think there is any need.

This particular case does not reveal much at all about propensity scores because it's so simple. In fact, the idea of propensity scores is to try to take a complicated situation with many background variables, here is just one gender, and create a display that's as simple and obvious as this display. So in this particular case, for propensity scores, I couldn't do anything other than what you

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have already done. The smoker is the outcome in this case in the sense that it's the exposure, yes/no, as you have indicated, or in some other kind of jargon, be the treated and untreated or treated and control groups. I may slip into using that jargon instead. So, the no smokers would be control and the yes smokers would be treated.

Q. And at times when you use X and Y variables I think in your supplementary report, would that smoker be the Y variable?

A. Not really. Smoker would be the treatment variable and Y would be like dollars.

Q. Okay.

A. Because the outcome you really care about and you try to estimate in some sense the effect of smoking on Y, on dollars, controlling for X gender and other things. So in this case that you have drawn, the only variable that we are trying to adjust for is gender. We are trying to compare like with like with respect to gender, which means we are trying to compare male smokers with male nonsmokers and female smokers with female nonsmokers, so in this case the propensity score formally, since there is only one X, is identical to that X, so X is gender, so in this example the propensity score is

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male/female.

Q. Does it have a value which is male/female?

A. Well female you wrote down as one and male you wrote down as zero so in this case the propensity score is -- is the zero one variable. The probability, it's the probability -- okay. If you are -- if you are male, the probability of being a smoker is two-thirds, if you are female the probability of being a smoker is one-third, so that would be the propensity score.

Q. Okay. So two-thirds for males and one-third for females?

A. Yeah, is the smoker/nonsmoker.

Q. Okay. And then as I understand it, you do something to figure out whether --

And that would be a mean?

A. That's right. Yeah. So the -- the average value -- all the males that propensity score two-thirds and all the females have a propensity score of one-third.

Q. Okay. Then you do something to compare the -- those two means.

A. Well basically those two distributions.

Q. All right.

A. Which in this case are the kind of -- kind of

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trivial because you look at the average value for the propensity score for males -- sorry -- for smokers and look at the average value of the propensity score for nonsmokers and see how far apart they are. More generally, you have, instead of just a zero one variable, you have a whole collection of continuous, like age, you would have lots of ages represented so there would be a distribution, but what you are looking at is the distribution of gender for smokers and comparing that to the distribution of gender for nonsmokers. So if you look at a histogram representing distributions, would you like to do that?

Q. Yeah, that would help me picture it.

A. Okay. So for smokers, here is zero and here is one, the two -- two values for the propensity score, and the histogram would go up one-third on zero and two-thirds on one because they are females and then for non -- I did nonsmokers. Sorry. Nonsmokers would be one-third and two-thirds and for smokers -- I'll do it down here. Smokers, it would be two-thirds and one-third. The histogram is --

In this case where there is only one variable, this is the histogram for nonsmokers, there is a histogram for smokers, and so that corresponds to

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these histograms on Trial Exhibit 2278, where instead of just having two values, zero and one, there are lots of possible values for the X variable, for the background variable.

(Interruption by the reporter.)

A. So here there are just two, so we are looking like that, so this is what? The top one is histograms for nonsmokers, so for the -- since only two possible values instead of however. There are a dozen different values here. You just have two -- a third, two-thirds and for the smokers you would have two-thirds, one-third, the two values.

Q. Right.

A. Is that clear?

Q. Well that's what I was thinking. If you had a histogram that looked like Trial Exhibit 2278, you would have some -- some bar like you drew here --

A. Correct.

Q. -- at one-third and some bar at two-thirds for smokers and another size bar at one-third, another size bar at two-thirds for the nonsmokers.

A. See if I'm saying this right. For the -- Yeah, because -- yeah, the way -- I'm sorry. That's probably a better way. Because in this case it doesn't make any difference how you indicate the

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variable. The problem -- the specific probabilities for these -- for these people who are male for being a nonsmoker is one-third and two-thirds.

Q. Okay.

A. And down here it's one-third and two-thirds. Let me look to see whether I have probabilities here, or it's called linear propensity scores, to make it clear. These are actual probability, so these are propensity scores.

In the supplemental report I talked about how -- that you can actually do the plots in the probability sale or you can do it in what's called the linear scale, which are the variables that went into them, and for reasons I described in the supplemental report, they are usually done in terms of linear scores, not the probabilities per se. But in this case it makes absolutely no difference being what the picture is.

Q. The picture looks pretty much the same?

A. Is the same. The males are coded zero and they all have a probability of one-third being nonsmoker and two-thirds being a smoker, for the females it's two-thirds.

Q. So if we do this sort of revised chart, we have the bars at one-third and two-thirds, is that the

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probability scale?
A. Yes, that would be the probability, because the -- among the nonsmokers, all the males have a probability. Let me try to be -- probably being -- You know what I'm doing? I'm switching around. The -- the probability for males of being a smoker, for all the males the probability is two-thirds.
Q. That makes sense to me.
A. Right. And the probability being a smoker for female is one-third.
Q. And that's what you have shown with these bars here?
A. Right.
Q. As I understand it.
A. Right.
Q. Okay. And then so that's the histogram that at least shows something like what we have seen?
A. Right.
Q. I can compare it to what Raghunathan had done in this Trial Exhibit 2278.
A. Correct.
Q. And then -- and then you went on, as I understand it, in the supplemental report and you get what's called the mean propensity score?
A. Right, the average value.

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Q. So can you show me how you do that in this example?
A. Okay. The -- the way I was thinking about it at lunch, I wasn't doing the probability scale because in the supplemental report I -- I do it in the linear form instead, so to make it more consistent with that I was just -- the way I was thinking about doing it is just having the zero one rather than the probabilities themselves.
Q. It will help me to understand it because I don't -- I sort of had some conception of what these histograms looked like, I thought, from example 2278. If you could keep that form --
A. I'll try to move more with the supplemental report notation.
Q. The methodology will be similar, just a different scale or something?
A. Correct. Yeah, correct. I was thinking about the other way. If I can take a minute to think about it this way instead of correcting it on the -- on the fly.
Q. Sure.
A. Maybe the way I wrote it here isn't quite right. Let me make sure it's right.
Okay. I'm just mumbling to myself now.

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MR. LOVE: We can go off the record.

THE WITNESS: Yeah, let's go off the record.

(Discussion off the record.)

BY MR. LOVE:

Q. So now you are going to show me how to get the mean propensity score?

A. Yeah, mean propensity score. Okay. And this I had backwards, too, previously these bars I had backwards, so the one on the right is -- is now correct. I was reversing the way we coded male and female.

Q. Okay.

A. Among the nonsmokers, a third of the people have -- among the nonsmokers --

Q. Uh-huh.

A. -- there are -- there are two-thirds of the people are females and have propensity score one-third, so the probability, if you are a female, the probability of you are a smoking is one-third, the probability that you are a smoker as male is two-thirds, so the two values of the propensity score are one-third and two-thirds.

Q. Okay.

A. Okay?

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Q. Does it make any difference which is on the left and which is on the right?
A. The only reason I did it, the value, these are values of propensity scores so they go from low to high.
Q. Okay.
A. Just as do here, go from low to high. That's what I was reversing before and managed to completely confuse myself in so doing. It was not intention but happens.
Q. Okay.
A. So this is the probability of being a smoker, is either one-third or two-thirds.
Q. Okay.
A. One-third if you are female, two-thirds if you are a male. Now among the people who are nonsmokers, two-thirds of them are females.
Q. Yes.
A. So this is -- Here is a distribution. So among the nonsmokers, there are two-thirds that are females.
Q. Yeah.
A. And one-third are males. The thing that's a little confusing is, two-thirds have value one-third and one-third have value two-thirds.

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Q. Right.
A. Okay? Now if you go among the smokers, the females that are with the ones with value one-third and the male are the ones with value two-thirds. All right. That's what -- that's males -- females have probably one-third propensity score of being a smoker, males are a probability of two-thirds being a smoker. Among the smokers, what proportion of people have value one-third? That's the females. I don't think the smokers, one-third of people are smokers, so the -- the proportion of -- among the smokers -- what did I say? I don't know if I said that last sentence correctly. What I meant to say was, among the smokers, one-third of the people are females, one -- therefore one-third of them have propensity score of one-third, and that's why this is one-third, this height of this bar, histogram, is one-third. Among the smokers, two-thirds of the smokers are male; therefore, among the smokers, two-thirds of the people have propensity score of two-thirds, and that's the histogram here for -- on the smoking group for males has -- rises two-thirds above the value two-thirds.
Q. Right, as high as the --
A. The females, where the reverse is true for the

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-- among the nonsmokers.
Q. Okay.
A. So this shows the difference in distribution and the propensity scores between nonsmokers and smokers.
Q. And these are the bar charts up on the right side of the page?
A. Correct. These two charts, the nonsmoker one corresponds to nonsmoker one in this trial exhibit, and the smoking one down here which has two bars corresponds so this one for smokers. I think that's -- this is a histogram for nonsmokers, histogram of smokers; correct.
Q. Okay.
A. Now there is a mean value for this thing called one-third and two-thirds, in this -- among nonsmokers, and there is a mean value for among the smokers, "mean" meaning the same as average.
Q. Uh-huh.
A. I don't think the nonsmokers, if I didn't screw this up, the mean value for this thing, one-third/two-thirds, is four-ninths, I believe.
Q. Is that --
A. I'll tell you how I got that. A third of the people have a value one-third, one-third times one-third.

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Q. One-third times one-third.
A. And two-thirds of the people -- I'm sorry, I didn't do that right.
Q. Okay.
A. Two-thirds times one-third, two-thirds of the people had value one-third.
Q. So that's two-ninths?
A. Correct.
Q. Two-ninths.
A. Correct. Two-thirds of the people had value one-third. That's two-thirds times one-third when added together --
Q. That's two-ninths again; right?
A. Right, so two-ninths plus two-ninths is four-ninths, so that's the mean value of the propensity score of the nonsmokers.
Q. Of the nonsmokers.
A. Correct.
Q. Okay.
A. Now let's go to the smokers. One-third of the people have value one-third. That's one-third times one-third, is one-ninth, and two-thirds of the people have value two-thirds.
Q. So that's four-ninths?
A. Correct. One-ninth plus four-ninths is

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five-ninths.

Q. So that would be the mean propensity score for the smokers?

A. Correct.

Q. Smokers, okay. And then in the first, I think, bench mark or test that you used you compared the difference in those means in some way?

A. That's right, the difference of the means, but standardized by the number of standard deviations because we could have -- Let me slow down here a second. In this display where it's 2278 --

Q. This is trial Exhibit 2278.

A. Right. I'm working on probability scale, and when I see this standard deviations away --

Q. SD Away?

A. Right. What I do is, I take the difference in those two means, which is one-ninth, and I divide it by the -- an average within group standard deviation.

Q. Right.

A. To say how -- to show how far apart the scores are in a -- normed by within group standard deviation.

Q. Okay.

A. With propensity scores themselves, that's not a critical thing to do, but it's critical when you are

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looking at -- through the linear form of the scores, which is the thing I do in the supplemental report, which is actually more relevant. Suppose I measured -- I called male zero and females 47,000, I get a completely different answer in terms of how far apart they are in means, just as I-- coding it zero one, they call it zero and 47,000, so you have to divide by some common scale factor. And so what I then do is look at how many standard deviations apart the groups are. That's the standard deviation away. And in this case, if I did the calculations right, the number standard deviations apart is one over the square root of two, or about point seven.

Q. Okay.

A. That's what I was checking, calculating both ways. It's a trivial calculation but I wanted to make sure.

So in this particular picture, you would have the number standard deviations between these two groups, would be point seven.

Q. Okay. The way you did that was taking the five-ninths and subtracting the four-ninths for the numerator?

A. Correct.

Q. And that's one-ninth for the numerator?

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A. Correct.
Q. You are going to take that one-ninth and divide it by the standard deviation?
A. Within a group.
Q. Within any particular group or --
A. Well in this case they are the same because of the -- of the symmetry.
Q. Okay.
A. And in general, though, in this output, this is defined to be the average variance in the two groups divide -- so the average is the variance here plus the variance there divided by two and then take the square root.
Q. So in a more complicated example, you take the two variances, divide them by two and take a square root?
A. Correct.
Q. But in this case since they are the same you can use one?
A. Yes, easy thing to do unless I blundered again in some way, but that's what I was trying to go on there.
Q. What you get when you have done that calculation is, you divide it by the standard deviation and the final result of one-ninth divided by the standard

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deviation is about point seven?
A. Point seven, I think, is the square root of two over two, I think is point seven, seven oh seven.
Q. Okay.
A. Now, so in this case what it is showing you is, there is a difference between the two groups. They happen to have the same standard deviation and they have the same sample size, and clearly what you would like to do in comparing like with like in this example is compare some outcome like expenditures or disease rates for the males and separately for the females, and that would be adjusting for sex. And because the distributions overlap in this case, you can do that, there are males, both males and females -- sorry -- both smokers and nonsmokers among males and smokers and nonsmokers among females. It's completely obvious from this example because it's so sample to see, because there is two levels for this background variable that you are drying to control for.
Q. But this shows us how to get the -- the standard-deviations-away figure for this little example?
A. For this little example, that's correct. But there is no payoff here calculating the propensity

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scores, it's probabilities. There is no payoff to doing that because you already know there are only two levels of the background variables you want to adjust for, and so it's completely obvious how far apart these groups are and whether or not you can adjust. So there is no payoff in doing the version of calculation we did, just look at it and we can -- we can calculate it if we want to but there is no benefit. I mean in a statistical sense. There may be benefit in understanding. I'm not trying to quibble. You would still end up within the males comparing -- you still end up within the males comparing smokers and nonsmokers and females compares smokers and nonsmokers and no need to do the propensity score analysis for any statistical purpose, although I perfectly understand why you want to do it here, for understanding.

MR. LOVE: Let's mark this as an exhibit so I know what we are talking about.

THE WITNESS: Shall I sign it?

(Plaintiffs' Deposition Exhibit 3551 was marked for identification.)

MR. BIERSTEKER: Do you think you will understand this more than a week from now? In which case, we could clean it up.

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THE WITNESS: Let me reinforce that. If you would like me to try and write out all the pieces to that to make clear what's going on in that example, I would be pleased to do so.

BY MR. LOVE:

Q. That might help when you have the time to do that but for now, just so we get -- I'll show what you we have marked as Exhibit 3551, which is the two-by-two chart that I wrote down and showed you before lunch and you have been telling me about propensity score calculations on; right?

A. Correct.

Q. What we see at the top is the two-by-two table; correct?

A. Yes.

Q. Over on the left side, below that, are these histograms --

A. Right side.

Q. On the right side of the paper is the histograms that you drew, and when you went through and did the final calculation on this one you have a mean difference of point -- roughly point seven standard deviation?

A. Correct.

Q. Professor Rubin, I'm just going to mark as an

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exhibit I believe the four court orders we had designated and are listed in the pre-designation. You mentioned you had looked at those briefly, at least, over the weekend; right?

A. Correct.

(Plaintiffs' Deposition Exhibits 3552 through 3555 were marked for identification.)

BY MR. LOVE:

Q. Professor Rubin, I'll show you what we have had mark as Exhibits 3552, 3553, 3554 and 3555, which are the four court orders that we had pre-designated for your deposition, ask if you recall seeing those over the weekend.

A. More or less, that's right. There is much that's unfamiliar --

Q. Sure.

A. -- jargon and methods, but I certainly do believe these are the ones that I glanced at.

Q. Okay. Let's go back to the first one, Exhibit 3552. Do you understand, Professor Rubin, that in this case the court has ruled that in measuring plaintiffs' damages that the parties must not factor in the mortality of smokers and nonsmokers?

A. In the sense that it's described it here, I

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believe, written the defendants and --

(Interruption by the reporter.)

A. In the sense that where it says here "WHEREAS, Defendants concede and Plaintiffs agree that the tobacco industry should not benefit from the premature deaths of smokers."

Q. Okay. If you look down two whereas paragraphs from that, do you see where it says "WHEREAS, Defendants claim that they must factor in the mortality of smokers and nonsmokers in order to challenge Plaintiffs' damages model"?

A. Yes, I see that.

Q. All right. And do you understand that the ultimate ruling of the court is that factoring in those relative mortality of smokers and nonsmokers must not be done in measuring damages in this case?

A. I think I understand what that -- what that means and I thought about what that means and, yes, I do believe I understand it.

Q. Can you tell me what that means to you?

A. Well that is, if I were to think about a person in the current world who is a smoker and what he would be like in either a world without smoking or in a world without alleged misconduct, and that if you -- now you track through time seeing what that

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person's healthcare costs would be under those two worlds, that if in fact under those two worlds, one real and one hypothetical, the smoker dies in the real world early and whereas he would have lived longer under the world without smoking either because of misconduct or otherwise, that no costs after that that the state would save can be brought in.

Q. So that's your understanding of the --

A. Correct.

Q. -- rule for damages in this particular case?

A. Pardon?

Q. That's your understanding of the court's rule for damages in this particular case?

A. Correct, correct, because it says you cannot take into account anything that would have the -- a premature, I think is the word, death of smoker, what I think of that as being death before he would have died in the absence of smoking, that after that event takes place, those costs of his being without smoking cannot be used to offset any other costs.

Q. And the next three orders, three exhibits, Exhibits 3553, 54 and 55, you understand that the court has determined that in this case that smoking-attributable expenditures constitute an indivisible injury that cannot be apportioned into

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how much of those expenditures would have been incurred in a world without defendants' misconduct, for instance?

MR. BIERSTEKER: I object to the question as mischaracterizing what the court has found.

A. That wasn't my understanding.

Q. What is your understanding?

A. I'm not precisely sure what it is, --

Q. Okay.

A. -- frankly. The fact is, when I think about it, what it might mean, is everybody does get sick and die at some point, and so there are expenses that people incur. I'm not -- I'm not exactly sure what the indivisible thing is. Perhaps I should be more aware of that but --

Q. Anyway, sitting here today, you don't have an understanding of what the court's ordering in these three exhibits?

A. Well I think I understand --

Q. Not the first one, the last group of three, which is Exhibits 33 -- sorry -- 3553, 3554 and 3555?

A. I thought I had -- Maybe we can take them one at a time.

Q. Sure.

A. If you are representing to me that of the next

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three all deal with that particular issue, then I guess I don't.

Q. Okay. Let's take the first one, 3553. Would you look at the bottom of page 7 and the top of page 8.

A. Fine.

Q. And read what the court has said there to yourself. By the "bottom" I mean the last whereas --

A. Right, last whereas.

Q. -- not the footnote.

A. Correct.

I do not have a clear understanding what means. Certainly it can't mean that anyone who is a smoker has -- all that burden, all costs go on to someone else, so I, no, I do not have a clear understanding of the meaning of that.

Q. Let's look at the next exhibit, 3554.

A. Yes.

Q. And if you look first at the very bottom of page 8 [sic. (6)] and carries over to the top of page 9 [sic.(7)], where the court says, "WHEREAS, the indivisible injury rule does not require Plaintiffs to show damages are 'fairly distinct'; the burden of allocating the damages falls upon those who claim it can be disaggregated," that phrase, is that --

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MR. BIERSTEKER: I don't see it on this page.

MR. LOVE: We have the wrong exhibit.

THE WITNESS: I have the wrong exhibit? You said page 8?

MR. LOVE: I meant to say page 6 and I think over to page 7.

THE WITNESS: I thought you said 8. I'm sorry.

MR. LOVE: Yes. I meant page 6 carrying over to page 7.

THE WITNESS: Yes.

Q. And if you will also look at footnote 8 near the bottom of page 7 where the court says defendants have not offered evidence --

A. Yes.

Q. -- do you see that?

A. I see, yeah, where they misquoted me, I see that.

Q. And then the final sentence of that footnote, "Plaintiffs here presented testimony that disaggregation would be impossible and not even Defendants' experts can identify a means of apportionment," do you see that?

A. That's based on a misquote, they are misquoting

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me, my deposition. I can read it but it's wrong.
Q. And then if you just look at page 8, the next page, the third whereas clause.
A. Yes.
Q. Second portion of that says, Thus, impossibility of apportioning damages in cases of this magnitude cannot be used as proof of lack of causation; if this were so -- if this were not so, plaintiffs in complex cases would not have any remedy for injuries suffered and defendants would be rewarded despite their culpability. Do you see that?
A. Yes, I do.
Q. Do you have an understanding what the court has ruled in this order as it affects calculation of damages in this case?
A. I believe I have some understanding. And there are a lot of, in my world, foreign technical words being used that probably have special meaning, but my understanding from these words is that the burden of proof on showing the effect of damages due to misconduct, that burden has been shifted to the defendants, I believe, and that saying it can't be done, it's impossible to do, is no excuse for not producing -- for trying to do it. That cannot be used as a defendant, in fact can't be done. I never

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said it can't be done.

Again, that's a misquotation of mine, so -- but it seems to be -- it seems to be a reasonable statement, the last clause that you read, "the impossibility of apportioning damages cannot be used as a proof of lack of causation." That's okay.

Q. All right.

A. I understand that.

Q. Just to complete the series of orders here, if we look at the last exhibit --

A. Sure.

Q. -- 355 -- lost my numbers --

MR. BIERSTEKER: 5.

Q. Five, 3555, and if you just look sort of in the middle of the page where it says, "MOTION DENIED."

A. Yes.

Q. Read further on. It says, "NON-LIGGETT DEFENDANTS' NOTICE OF MOTION AND MOTION FOR MISTRIAL OR LEAVE TO SUPPLEMENT EXPERT REPORT."

A. Yes.

Q. Now do you have any understanding as to the fact whether defendants offered to produce any damages models that did disaggregate smoking-attributable healthcare expenses into those that arguably were caused by defendants' alleged misconduct and those

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that arguably weren't caused by defendants' alleged misconduct?

A. Yes. My understanding is that some initial work was done by someone for the defendants.

Q. Did you have any involvement in that?

A. No, I did not.

Q. Have you reviewed that effort?

A. No, I have not.

Q. You haven't seen Dr. Wecker's second supplemental report --

A. No, I have not.

Q. -- dated March something, April something?

A. I believe I may have had a two-minute conversation with Peter Biersteker about it at one time but that's all. I have not seen any documents, I have not talked to Wecker.

Q. Do you understand the court ruled defendants could not introduce those models into evidence?

A. Yeah, although it seems kind of contradictory. I understand that means they can't, although from this it says they must, but I'm not a lawyer so I don't know what's going on. They seem to be sort of contradictory, you must do it but you can't do it, but that's not my field.

Q. If in fact the court has determined that no

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party may introduce any model that attempts that disaggregation, would you be able to follow that rule in this case?

MR. BIERSTEKER: I object. I don't know what you are trying to do. I mean, you know, it's clear that -- it's clear Professor Rubin is not a lawyer, it's clear that he has an impression because you asked him to read stuff about some of the things that he has read, but I think we are probably well beyond the scope of the expertise and the scope of the second supplemental report that he submitted. I don't know what you are trying to do but I think these questions are -- this is sort of can you follow the rules if the court tells you what the rules are, and the answer to that question is going to be yes, we follow the rules once the rules are clearly spelled out and the court tells us what the rules are, but I think -- I think you are pretty far afield.

BY MR. LOVE:

Q. Professor Rubin, let me ask you a different question, then. Having seen these court rulings, have you had any opportunity to talk with them, to get a better understanding by talking to anybody about them? I'm not asking what the conversation was

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but have you had a chance to do that?
A. I've had brief conversations with Peter Biersteker about the meaning of them but I certainly can't see myself being in a position where I have to make some legal assessment of whether I can answer a question or not. I will answer all questions honestly that are put to me and if a question comes up that shouldn't be asked, presumably it's up to someone else to decide whether that question should have been asked.
Q. After having seen the orders and having a conversation with Mr. Biersteker, did you go back and examine any of the opinions you have expressed in your expert reports in this case to determine whether or not those orders would affect those opinions?
A. Well it -- not affect the opinions in general at all but in the -- the issues that are addressed, yes. Shall I try to clarify that?
Q. Yes, if you could.
A. When I wrote these -- the original report, opinions about issues of causality, there were two kinds of causal questions I brought up. One was the ever smoking one and then there was the smoking -- damages due to alleged misconduct, and in one of those examples, or maybe in both, I don't remember,

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in discussing those topics, I used examples, and one of those, the examples, it had this, quote, death credit going in it. It had it running where the smoker died earlier, I think if he were without smoking would be in a nursing home and cost more. Now that example still works for the issue of causality. There is no problem with the example. But the point is, the court has said I cannot consider savings in costs that occur after this person would have died as a smoker, so what I would have done or could have done or should have done when I now think about it is the way I mentioned earlier in today's deposition, that I would have decomposed these two streams in the future to not carrying them on until he died, this person died under either scenario, that he kept going as long as he is alive in one scenario. Now the issue is, I want to stop as soon as the person dies under either scenario and only look at costs, the stream of costs up until the point in which he is alive under both scenarios. So, it would -- it would -- it would have changed my decision on how to address that causal question by decomposing the costs into two pieces, one piece which is when this person is alive under both streams -- by "streams" I mean smoking and nonsmoking

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streams and thereafter -- and the court has said those costs thereafter may not be used in any calculation. And that would work for both the ever smoking and the misconduct version of the causal question.

Q. Is the example you are talking about the one that appears on page 7 of your first expert report, trial Exhibit 2273?

A. Yes, that's the example I had in mind.

So now what I would do, I would say, okay, here's the streams under world with smoking, here's the stream under world without smoking, and the court has decided that what I have to do is divide the stream into two pieces, before 1980 and after 1980, and the costs in the world with smoking in this little example after 1980 are zero and the world without smoking, they -- they build up, and the court has said you have got to stop counting dollars in 1980. You can count those if you want but you can't use them in this trial, they can't be used to trade off, so instead we have to start up with the date of alleged misconduct, which I believe is this example. I don't remember exactly, maybe it's 1955. No, average smoker. I'm sorry, average smoker. It's right.

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Q. This example doesn't involve misconduct.
A. Now I remember that's right. This is ever smoking, so you build up all the healthcare costs from the time he was born until the time he died with the world with smoking and build up all the costs from the time he was born until the time -- in a world without smoking, because -- until he would have died with smokers. You have to shut him off at 1980. You can't account any of those costs after that. It's a legitimate question but the court has ruled you can't use those costs in calculating those things. I understand that. That's a ruling the court has made.

So now in hindsight, that example would have been clear if I had said that then, so that there are costs before that date and costs after that date, and there may be reasons why you would like to ask two different questions, not one just total costs but one total cost up to the point of demise.

(Interruption by the reporter.)

A. Up to the point of demise, death.
Q. If you look at the example you gave on page 3 of your first report, Trial Exhibit 2273, can you tell me if that example is affected at all in your mind by any of these court rulings we have just discussed?

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A. The example isn't but it -- it may be that aspects of -- apparently aspects of trying to build the model to address that question apparently were not allowed. I think that was the last court order you showed me. So that the first attempt to try to do this was not allowed. I have no idea whether that means all attempts to try to answer the question that's being asked will be thrown out. That's for you guys to decide with the judge, or you guys divided with the judge.

Q. Is it fair to say you don't know whether these orders affect this example on page 3 of Exhibit 2273, not the --

MR. BIERSTEKER: Wait a minute. Can I ask that we segregate out the Exhibit 3552, which is the one that deals with the so-called death credit, from the wrongful conduct orders? Because I think -- Well I would ask that you do that if you don't mind. Otherwise, it's ambiguous.

Q. Let me ask you first, Dr. Rubin, do you believe that the example on page 3 of Trial Exhibit 2273 involves the death credit as we discussed?

A. No. Because there is no death in the example.

MR. LOVE: Does that satisfy you, Peter?

MR. BIERSTEKER: It does.

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Q. Now the question I'm asking is: The other three court orders that didn't deal with death credit at this time, that was the first order, but the three after we talked about, is it fair to say you don't know whether any of those three orders affect your ability to use the example here on page 3 at the trial?

A. I would not -- You are asking sort of a legal question. I can give you my lay interpretation of the legal question.

Q. I'm just asking what you understand what you are committed to do and not committed to do at trial.

A. I believe I'm still allowed to -- If the question has to do with misconduct, this is a -- is a way of thinking -- the way of thinking about misconduct, and my lay understanding of what the court has ruled has been more of a burden of proof if you are going to do this analysis, the plaintiffs don't have to do it, and I guess secondly that the attempt to do some version of this and submit it was denied. And that's my total understanding.

Q. Okay.

A. I don't know why -- I don't understand why it was denied, I don't know whether that denial means no further attempt can be made. I have no idea.

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Q. That's why I say it's fair to say you don't know whether that ruling would extend to the example here on page 3 of Exhibit 2273?

A. I guess I don't see how it could extend to that but again that's my naivete.

Q. Is there anything in your second -- your supplemental report, the one dated January 10 marked as Exhibit 3546, that you have gone back and thought about in light of the four court orders, either the death credit or these other three court orders?

MR. BIERSTEKER: Object to the characterization "death credit." Go ahead.

A. I don't think so, except that the -- the second part where you talk about possible data sources for behavioral changes.

Q. You are referring to section II of the report that begins on page 8?

A. Section II for causal modeling which addresses behavioral changes as possible -- as a possible result of alleged misconduct. If someone said you are not allowed to talk about anything about misconduct, you are not -- and therefore they also said you are not allowed to talk at all about behavioral changes as a result of misconduct, then I guess those would not be germane. The propensity

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score stuff is completely relevant.
Q. And not affected by court order?
A. Not affected by court order at all.
Q. As far as you understand.
A. As far as I understand, yes, that's right.
Q. Have we now exhausted your -- any new opinions you may have come to after reviewing the four court orders?
A. As a result of reviewing the court orders, you mean?
Q. Yes.
A. Yes, I think we probably exhausted my knowledge of the law on such issues.
Q. In that pre-designation of documents for your deposition, Exhibit 3547, if we look at item number 12, Heart and Stroke Facts, it says, 1966 statistics, Statistical Supplement, in this pre-designation. I'm sure that's a typographical error, supposed to say 1996.
A. Gee, I read the '66 one pretty carefully. That's not true.
Q. The document listed below it, CN 000046, is in fact the 1996 statistical supplement, so I'm sure you got the right one; correct?
A. Probably did. I did not check the date.

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(Plaintiffs' Deposition Exhibit 3556 was marked for identification.)

BY MR. LOVE:

Q. Professor Rubin, I'll show you what we have marked as Plaintiffs' Exhibit 3556, the American Heart Association document entitled "Heart and Stroke Facts: 1996 Statistical Supplement," and ask if that's a document that you reviewed in preparation for your deposition.

A. I did glance through this, yes.

Q. And then in particular if you look at page number 16 of the document. There is page numbers in the lower left corner.

A. Where is page 16? Sorry.

Q. Sixteen.

A. Yes.

Q. And if you will see the first subtitle is "Cigarette/Tobacco Smoke" in the left column.

A. Yes.

Q. And the third bullet point says, "Smoking-related illnesses cost the United States about $50 billion annually in medical care"?

A. Yes, I see that.

Q. Did you see that in preparation for today's deposition?

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A. Yes.
Q. Do you have any opinion about that statement?
A. I find most of this sort of -- It's not anything like a scientific document. I mean, just look at page 1. It says, "If a heart attack doesn't kill you, you will recover and be fine." I've noticed that. And then it says -- it has other information that says at the bottom just about right after that, "About two-thirds of heart attack patients don't make a complete recovery." Now how do those two things jive? You will be fine but you don't make a complete recovery? So I don't know what -- I don't know what -- it's something like an advertisement so it's -- I don't know what to make of it.
Q. I take it you have no opinions about this document.
A. I read the -- I read page 1.
Q. Right.
A. And my reaction was, "come on," so I have no opinion other than reading and sort of saying, well, it's words. They may get their numbers from somewhere but who knows what they mean.
Q. Having reviewed this document, have you come to any further opinions about your work in the Minnesota case?

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A. None.
Q. Item 13 -- Let's skip down to item 14. The Milliman & Robertson health risks and their impact on medical costs, item 14 in the pre-designation of documents for your deposition. Had you seen, reviewed that in preparation for today?
A. Yes, I did, quickly again.
Q. I'll be happy to show you the document if you would like but it may save time if I just ask you did you come to any opinions about that document based on your review?
A. Well it's -- My memory of it is it's a very simple descriptive presentation of risk factors and how they relate to healthcare costs, and it's indicated that here is a risk factor called drinking, which is -- which is always good for you, and they have other risk factors, have diet, other risk factors, cigarette smoking, and they just sort of go one by one, saying how good or bad they appear to be for you in the very simple descriptive sense. I mean I could try to point out some things if you would want to give it to me.
Q. If you have opinions about this study, I'd like to know what they are, so I'd be happy to show it to you.

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(Plaintiffs' Deposition Exhibit 3557 was marked for identification.)

BY MR. LOVE:

Q. Professor Rubin, I'll show you what we have marked as Plaintiffs' Exhibit 3557, a study by Milliman & Robertson entitled "Health Risks and Their Impact on Medical Costs." Is that the report you reviewed in preparation for your deposition?

A. It certainly looks like it.

Q. If you -- if you came to any opinions about this report based on your review, please tell me about them.

A. About the report.

Q. If you did.

A. About this report.

Q. Yes.

A. Opinions about this report.

MR. BIERSTEKER: This is fine. I'm just going to interject and say I don't mind you asking that question but if there is a specific portion you do want to ask him about, I think in fairness you ought to do that. If you generally are interested in knowing what his reaction to it was having quickly reviewed it, I don't mind that question; but if there is a specific thing you want to ask him about, I

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think you ought to do that.

Go ahead.

A. Okay. For various risk factors such as smoking, weight control, exercise, alcohol use, driving habits, et cetera, they -- eating habits, they do a calculation of four people with low risk on this risk factor and elevated on the risk factor, having or not having it, what it does to total monthly claim costs and hospital and patient days and I guess some other services, physician services, and they show that in the sense of elevated risk, currently smokes, has higher costs. They show that weight control is probably about the same, maybe more serious for some things, less serious for others. Exercise is less related than smoking. It's good to have the risk factor of using alcohol, they have some discussion about why that's -- why they don't like that, a pejorative way of saying it, but why they consider a risk factor something not to do despite the fact that the simple analysis shows it is a factor. And driving habits, they have a similar analysis. Eating habits are about like smoking roughly in this marginal sense. Stress is not good for you in this sense, mental health is mildly not good for you, and they just go through and show each of these one at

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time, how it relates, I guess, it's costs and hospital stays and use of physicians, so it's -- it's very -- it's a course summary of how these one at a time relate to these kinds of expenditures.

Q. Do you have any more detailed opinion or further opinion about the portion of report that deals with smoking as the factor?

A. More detailed opinion about --

Q. Than you already told.

A. I don't think there is anything new in here relative to all the other material that's been presented. I don't think it adds anything new if that's what you are asking.

Q. I just wanted to know do you have any specific opinions about their discussion of smoking as a factor in causing expenditures and hospital stays and so on.

MR. BIERSTEKER: Object to the form of the question.

A. They -- they do the most simple form of analysis and this -- smokers claimed to be higher than nonsmokers and so forth but it's not controlling for anything, just a very simple description.

Q. Did you -- You told me earlier you reviewed the trial transcript of Dr. Zeger and Dr. Wyant; correct?

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A. Yes, I did.
Q. Do you recall either of them talking about this particular study in their trial testimony?
A. I believe that Wyant referred to it. I'm not -- I don't remember whether Zeger did or not.
Q. I believe certainly Dr. Wyant did and we can leave Dr. Zeger out, at least for the moment.
A. Okay.
Q. Do you recall Dr. Wyant using this study and its results to compare to the numerical results of the Zeger-Wyant-Miller work in this case?
A. I don't remember that specifically, no, I don't. I'm not saying it's incorrect. I have no specific memory of referring to this study. I do remember he referred to things for support for his position but I don't remember this study in particular. Actually, do I have -- Perhaps. I really can't say whether it was this study.
Q. Okay. Do you recall Dr. Wyant comparing the Blue Cross results with a Milliman & Robertson study of people at Honeywell?
MR. BIERSTEKER: I think this is Chrysler.
Q. People at Chrysler. Sorry.
A. Not with that level of specificity I don't.
Q. You don't recall him looking at the percentage

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of extra costs due to smoking in his work compared to the percentage of extra costs due to smoking in the study we have marked as 3557?

A. Again I remember the general statements where he compares things but I have no specific memory of comparing to this particular study.

Q. Do you have any opinion as to whether this study can be used as a point of comparison with the results obtained by Drs. Zeger, Wyant and Miller.

A. It seems so.

Q. At least for the Blue Cross population?

A. It seems so, completely unconditional, on comparing like with like, that -- which is the -- what I think everyone I think agrees for getting smoking-attributable expenditures. I don't see why it lends much support at all.

Q. If you have one study that compares like with like, at least to the extent that Drs. Zeger, Wyant and Miller did in their report, and another study that, as you say, doesn't make -- isn't much of a comparison to like to like and yet they -- if they came out with very similar results, would that mean anything to you?

A. Yes. Quite possibly in this case it means when one tried to adjust for the factors, didn't do a good

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job.
Q. That's one possible?
A. That's one possible. And because of the propensity score analyses, I suspect that's probably one of the major reasons why.
Q. And any other meanings?
A. Pardon?
Q. Any other significance that would have to you?
A. That all those background characteristics that people say should be controlled for and that in other cases people say even a bigger list should be controlled for and they are relevant, that they should be controlled for and they should make a difference because supposedly they are related, and it doesn't seem to -- you still get roughly the same answer. It just doesn't lend any support, to me.
Q. Do you know if in the Milliman & Robertson study they controlled for age and gender?
A. They controlled for age and gender. I'd have to look at it to see whether they did. I don't believe they did but it's possible that they did do it. I would think they would control for gender but now that you mention it, it doesn't -- I don't see it here. Do you want me to glance at it and see whether I can figure it out?

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Q. Sure. It might help you, Professor Rubin, if you look at the bottom of page 9.
A. The copy I have, I can't read the numbers. Here it is, 9. Demographic characteristics?
Q. Right, near the bottom of the first -- the left-hand column.
A. Okay.
(Discussion off the record.)
A. Okay. Yes. Evidently they did adjust by gender and by some categorization of age, which at least they don't say here on the bottom of page 9 how they did the age adjustment, whether it was the 35 to 60 and 65 and older or some other categorization. Perhaps you can point me to where they say that and I'll try to then understand what they did.
Q. At the moment, professor, I'm not sure it says in the report itself how they adjusted for age so I can't point to anything at the moment.
A. Okay.
Q. Is it -- Knowing there is some adjustment for age and also adjustment for gender in the Milliman & Robertson study, is one possible reason that its results are similar to Zeger, Wyant and Miller's results is that the other factors besides age and gender don't matter that much in relation to smoking

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and healthcare expense?
A. That's a possibility but I think it's -- I think it's relatively remote based on all the discussion of the need to compare like with like and control for this -- these other variables, for example in this model that Harrison I mentioned to you has 38 variables there he thinks must be controlled for.
Q. Is your opinion based on what other people like Harrison have done or is that based also on your own expertise?
A. Well my -- My opinion, relying on other people who know about how these other risk factors are related to disease and healthcare costs, I'm relying on other people for that and the fact that here we see eating habits is a bigger risk factor than smoking, et cetera, so we are relying on other people to make those kinds of statements. I'm relying on myself with respect to how one does adjustments for those kinds of factors appropriately in a situation where there are these background differences between the groups. That's like the propensity score thing to get started. You can't rely on linear models and you can have examples, not just linear models, models of the kind they are using, because it's quite possible that a good adjustment would result in a

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real change of the numbers. And that's based on work that started in my Ph.D. thesis under Bill Cochran in the late '60s, so I guess that's 30 years of work on this topic that suggests it can make a substantial difference how you try to adjust for variables like that that have the different distribution between exposed and non-exposed, smokers and nonsmokers. And if you don't do a good job of that, in terms of "good" being better techniques, better looks at the data, you can get nothing happening. It's completely unreliable.

Q. Is it fair to say, then, your expertise is primarily in the area of how to adjust for background variables and other variables as opposed to choosing which variables should be included?

A. Absolutely, absolutely, that's fair. That's why I've said earlier with respect to the collection of variables that have been selected by the Zeger et al team, I presented propensity score analyses. The propensity score analyses accepted that collection of variables, saying if those are the variables you want to adjust for, my expertise is telling you whether you have, what are the dangers and how to do it better, how to do it well and when to be worried. My expertise is not in saying which of those variables

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should be selected. They did that, the Samet-Zeger team.

Q. If you look down at item number 17 on your pre-designation of documents for this deposition --

A. Yes.

Q. -- you have the Nurses Health Study. Do you recall seeing that?

A. I glanced at it, yes.

MR. LOVE: Let me mark that.

MR. BIERSTEKER: Let's take a break. Is that okay?

MR. LOVE: Sure.

(Recess taken from 3:00 to 3:15 p.m.)

(Plaintiffs' Deposition Exhibits 3558 through 3559 were marked for identification.)

BY MR. LOVE:

Q. Professor Rubin, I'll show you what we have marked as Exhibit 3558 -- it's an article entitled "RELEVANT AND ABSOLUTE EXCESS RISK OF CORONARY HEART DISEASE AMONG WOMEN WHO SMOKE CIGARETTES" -- and ask if you saw that in preparing for your deposition today.

A. I saw it, I glanced at parts of it. I did not read it all.

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Q. Did you come to any conclusions about this report?
A. It's another example relating ex -- excess risks, in this case coronary heart disease, relating that to cigarette smoking. Nurses Health Study, I guess what it's called. I guess that's what it's called.
Q. Yes, commonly called the Nurses Health Study, I believe. And you sort of described what it's about. My question is: Did you come to any conclusions about this report when you -- just any conclusions at all about it, I guess.
A. No conclusions that would have changed any of opinions or affected the reports that I wrote.
Q. Did you come to any conclusions about whether this is something that Professor Zeger, Wyant and Miller could take into consideration in estimating damages in this case?
MR. BIERSTEKER: I object to the form.
A. State that again because I'm not quite sure.
Q. Sure. Did you come to any conclusions about whether what's presented in this Nurses Health Study, also trial Exhibit 16,039, I guess, is something that Professor Zeger, Wyant and Miller could reasonably take into consideration in estimating damages in this

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case?

MR. BIERSTEKER: Object to the form again.

A. It's so subjective I don't know how to answer it, whether I know whether they could reasonably take into consideration.

Q. Whether you think it's something anyone trying to estimate damages in this case reasonably take into consideration.

A. Well in an indirect way, yes. I mean you could look at this and see what kind of other risk factors they -- they want to control for, to identification of the kinds of variables that you might want to look at just in the same way I referred to other documents, other analyses where people have said here's the list of background variables you want to adjust for in order to get a SAF or attributable expenditures, you could look at this to see what they think you should control for to try to cover this.

Q. That's as far as your analysis of this report went in this case; correct?

A. Correct.

MR. BIERSTEKER: If you have a specific portion you would like to ask him about, you may.

MR. LOVE: No. I think that's fine, Professor Rubin.

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THE WITNESS: Okay.

Q. I'll show you what we have marked as Plaintiffs' Exhibit 3559. It's an article titled "State Estimates of Medicaid Expenditures Attributable to Cigarette Smoking, Fiscal Year 1993," by Leonard Miller and other authors, and ask if you reviewed that in preparing for your deposition today.

A. I reviewed this in something like the way I reviewed the previous one.

I should have said in the previous one I didn't really read it very carefully so it could be there is some things in there that would be more revealing, but I read through it very quickly. I didn't see anything.

Q. That's referring to the Nurses Health Study?

A. That's right. I just wanted to clarify my answer to the last question.

Q. Now we are on to Exhibit 3559.

A. Correct.

Q. And you also read that?

A. Quickly.

Q. Very quickly?

A. Right. Because it's -- That's right, I read it, read it quickly.

Q. Did you come to any opinions about this report

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that are different than the opinions you have expressed about the Zeger-Wyant-Miller reports in this case?

A. Well the analysis is only very cursorily described here and it -- it really didn't generate anything new and different to me than the issues that have already been discussed.

Q. Are you --

A. Again, if you want to refer to something in particular, I may be able to address it, but it's a version on the same thing that we have seen a lot of.

Q. Are you familiar with the publication that this appears in, Public Health Reports?

A. No, I'm not.

Q. I take it having reviewed this exhibit quickly over the weekend you -- it hasn't changed any of your opinions or given you any insight into any new opinions in the Minnesota case?

A. Correct.

Q. Thank you.

(Discussion off the record.)

Q. Professor Rubin, in your previous deposition back in October of this case, you were asked questions about the Framingham study. Do you recall

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that?
A. Yes.
Q. And --
A. Not with great specificity but I recall in general.
Q. And that study was marked during your deposition and it's also marked as Trial Exhibit 2269. That's the document I'm referring to, sir.
A. Yes.
Q. My question today is whether since that deposition in early October 1997 you have done any further review or analysis of the Framingham study.
A. No, I have not done anything. My memory is that we looked at the -- that there were two issues that arose in that deposition. One was probably how to handle missing data, and they don't in a way that -- my memory wasn't particularly satisfactory, nor did they do any comparison of the adjustment for the background variables between the smoking and nonsmoking groups. But that's my memory from last. This is the first I've seen it since that deposition and I haven't read it before the deposition, either, so this is just my memory from having seen it then.
Q. Do you recall from reviewing the trial transcripts of Dr. Zeger, particularly Dr. Wyant,

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that they, Dr. Wyant at least, used the Framingham health study as a point in comparison to the work he did in this case?

A. Yes, I remember seeing that.

Q. And my question is: You had not reviewed, first of all, the Framingham health study since seeing that trial transcript?

A. Correct.

Q. And I take it you don't have any opinions about the way in which that study was used as a comparison by Dr. Wyant?

A. Well in some sense, yes, because it did not -- my memory is that this paper does not do any comparison of the distributions of background variables between smokers and nonsmokers.

Q. Like propensity score analysis?

A. Anything to just reveal how different they would be, that's correct, like the propensity score analysis is a very efficient way of doing such a comparison, so they are relying on the models to the adjustments. I don't know from this article how far apart they are, but from the supplemental analysis that I did use in propensity scores in the supplemental report, which makes it look like there really are substantial differences between smokers

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and nonsmokers and many background characteristics, so that you can't trust those kinds of models. I guess I'm now even more dubious about whether this article, for example, did in fact do a control for those -- reliable control for those background variables. There is no evidence that it did, that it looked to see what problems might be there.

Q. Is it your opinion that any study that compares smokers and nonsmokers first must do something like a propensity score analysis to compare the distributions between smokers and nonsmokers before making that kind of comparison?

A. If the desire of the analysis is to control for these full collection of background variables, then it's imperative you look to see whether such a comparison is even feasible, and then if it's feasible, how sensitive it might be to linear modeling or log-linear modeling assumptions --

(Interruption by the reporter.)

A. -- instead of just throwing the data into a computer program and pushing a button and seeing the answers that come out.

Q. Any other opinions about using the Framingham health study as a comparison to the work of doctors Wyant, Miller and Zeger in this case?

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A. No.

One more point of clarification perhaps on the question before that last one, your question was worded in the comparing smokers and nonsmokers and my answer really is not only comparing smokers and nonsmokers but just generally when comparing an exposed group and unexposed group, and you are trying to make that comparison controlling for a collection of background variables. You have to look to see what the distribution of background variables is for the exposed and non-exposed groups so see whether such a comparison can be made and how sensitive the adjustment might be to modeling assumptions in general.

Q. Now you had -- in your answer said something about looking to the distribution of the background characteristics between the smoker group and nonsmoking group, first to see whether any comparison can be made, whether it's feasible, I think you said.

A. Yes.

Q. And is there some test in propensity score analysis that must be satisfied before any type of comparison of the two groups can be valid or feasible?

A. Let me address that with a trivial example

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first. Let's suppose you want to compare smokers and nonsmokers adjusting for gender, sex, and you found that all the smokers were males in the data set and all the nonsmokers are female, and they say, okay, now I will adjust for sex. Well that's -- obviously you can't adjust for sex. The smokers are males and the nonsmokers are females. Let's suppose instead that they were almost all males, there was just one female who was a smoker and there is just one male who is a nonsmoker, and obviously it becomes more difficult to rely on those results because there is noise. Now that's a simple case where there is one variable like the one we had in this former exhibit. In complicated situations, there are many variables and the way in which they differ can be much more complex, and to understand the ways in which those distributions overlap or don't overlap, to understand that, you have to do some analysis to see to what extent you can -- you can adjust for those kinds of differences.

Another example would be if age is the background variable and all the smokers were between 18 and 35 and all the nonsmokers were between 34 and 65, to do a linear modeling kind of adjustment or log-linear modeling kind of adjustment means drawing

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straight lines and reading the data pretending you know what's going to happen, so it's a complete extrapolation. You have to look at those kinds of background differences and understand to what extent those adjustments are feasible and to what extent they rely on assumptions that you might be willing to make, but you have to explore the sensitivity of the answers to other assumptions, at least understand -- you have to understand what you are doing, not just put it in a computer program and push a button.

Q. Maybe it will help if we actually look at your supplemental report.

A. Sure.

Q. If I can find it in the pile here. It's Exhibit 3546.

A. Okay. Yes.

Q. And for instance on page 3.

A. Yes.

Q. Near the bottom you have a paragraph labeled number 1.

A. Right.

Q. And that's one of the conditions that you say you must obtain --

A. Right.

Q. -- or exist in order to use linear regression in

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comparing two groups like smokers and nonsmokers; is that right?

A. Correct.

Q. And here you say the difference in the means of the propensity scores of these two groups being compared must be small; correct?

A. Yes.

Q. And then --

A. Unless --

Q. And then in parentheses -- we will get there, I hope.

A. Okay.

Q. Then in parentheses you say, e.g. To me, that means for example. Is that what you meant by "e.g."?

A. Yes. For example, yes.

Q. The means must be less than half a standard deviation apart.

A. Correct.

Q. And then your situation as being benign and give three conditions, as I understand it, that must simultaneously exist for the situation to be, quote, benign, end quote?

A. Right.

Q. Is -- is this standard deviation being less than

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-- well start that question again, professor.

Does the difference in the means of the propensity scores being less than half a standard deviation apart, is that test half a standard deviation apart or is that an example of what you mean by "small"?

A. Just an example of what I mean by "small" because it depends on other kinds of distribution characteristics, how skewed things are, other things. It certainly -- Certainly I would never do a test, for example, are the means less than half standard deviation apart, and if the test says I believe they are, then you are fine. That's not good statistics. These are guidelines. They are not hard-and-fast rules.

Q. And if the difference in the propensity means, the propensity score is greater than half, it still might be appropriate to use linear regressions in comparing groups like smokers and nonsmokers?

A. Yes. If -- it can -- Yes, it can be, and these are -- these other conditions help make it easier. The work that I've done that's reported, the early work reported in here I did jointly with Cochran, suggests that if you have these symmetric, normal distributions with equal variance in the two groups

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and no one group is bigger in size than the other by very much, that -- those linear extrapolations tend to be pretty reliable.

Q. Let's assume we don't meet the three, all three of the conditions to make something which you call benign --

A. Uh-huh.

Q. -- on page 3 of your supplemental report, so we don't meet those three tests simultaneously.

A. Right.

Q. Is there some upper bound on the difference in the means of the propensity scores between two groups being compared above which you say you just can't make the comparison, it's not feasible?

MR. BIERSTEKER: May I help?

MR. LOVE: Go ahead.

MR. BIERSTEKER: I really mean, you mean feasible at all using any method or feasible using only regression methods?

MR. LOVE: I mean feasible without using any method.

Q. That's what I understood you to say before.

A. That's right.

Q. Did I properly understand what you said before?

A. Yeah. See, that depends a lot on the setting.

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Let's suppose, to take a real setting where there is an exposed group that's very small, some severe exposure, and they have background variables measured on them and the control group has all the background variables measured on them but it's huge by comparison. The example I'm thinking about in my mind is a study, for example, of 200 kids who are exposed to barbiturates in utero, the danger study involved in a group for many years, and there are 200 exposed but there are 8,000 potential controls, and when you look at the distribution on propensity scores and things like that, you find out that the groups are widely separated. Women who got barbiturates when they were pregnant tend to be different from women who do not get barbiturates when they are pregnant. These are women in the '40s and '50s, I think, long time ago. But because the 8,000 group is so large, there is a tail of that distribution who look like the exposed, the barbiturate, so among the 8,000 who did not get barbiturates, there is a subgroup of hundreds of them, very small proportion but that overlap the distribution of the exposed and look like them. There is a setting where you have, I believe, that maybe the standard deviation, the difference in --

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between means and units of standard deviation was three quarters, one, maybe even larger. I don't remember exactly now. But because it's ratio of the control pool to the treatment pool, you are interested in the treated people, the exposed people, and there are a lot of people down there in the pool, so you can make the comparison. If the two groups had been the same size -- another point also, the variance, I believe, in the control pool was greater so they had a long tail anyway. Not only were there many of them but they had a longer tail coming in with the variance, but instead they had the same variance and same sample size. We would find them very far about apart and would be very difficult to draw any conclusions from that study. That's an example where the technique used was matched sampling, where you matched each person who is exposed to somebody who is not exposed based partly on propensity scores, based partially on some other technologies to pair them up and set them aside. And then you throw away the irrelevant people from the control group because they just aren't like it, are not like the exposed people.

Q. Is it fair to say that in that study that you are referring to, is that the Danish study?

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A. Danish data set.
Q. Danish data set. That the distributions of background characteristics in the two groups, the exposed and unexposed, were not nearly symmetric?
A. I don't know if they were symmetric or not.
Q. What I'm trying to find out, Professor Rubin, is whether that particular example would meet your test for benign, because you said the second part of the test, the distribution to the background characteristics in both groups have nearly the same variance, I think you said that was true.
A. That would be -- That's right. That would be -- That would not be benign but -- and regression adjustment, we didn't do regression adjustment in that study. That would be disaster.
MR. BIERSTEKER: That's why I asked to clarify the point. You are using these points for feasibility generally when they are addressed to feasibility with regard to the progression. I'm sorry.
Q. That is my question. I did mean to ask is there some point where the difference in the means of propensity scores expressed in standard deviations apart gets to be so wide that no statistical means is available to make a valid comparison between the two

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groups in your opinion?

A. I gave you an example where they were quite far apart yet something that could be done, not by regression analysis but using these other matching techniques, because there is a little subgroup of people who really did look like the exposed and we could pick off those and throw off the rest. If we did a regression analysis in that study, it would have been a disaster, and there are analogous studies in economics, for example, where that's been documented, where they had a parallel randomized experiment and used regression to do these type of adjustments, and the regression methods are completely unreliable with respect to getting the real answer but the propensity matching methods, matching in general, got much closer to the randomized experiment answer.

(Interruption by the reporter.)

Q. My question, Professor Rubin, was: Even using these other techniques, does there get to be a point where, I don't know, where the difference in the means is two standard deviations or four standard deviations? Is there some point where in your opinion there is a cutoff and you say really no statistical methods can be used to compare those two

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groups in a valid way?
A. Yes, if the groups don't overlap. If you have an exposed group here and the control group has no people who look like the exposed, you can't do it. You can't do it using any technique.
Q. If there is no people who look like it.
A. If there are no people, that's my example of all the smokers are male and all the nonsmokers are female. Ah! Here is an answer that adjusts for sex. You can't do it.
Q. But I take it, then, there is no standard in terms of how many standard deviations apart the two means are that you can use as saying, there, once you get past that number, you can't use any statistical technique to compare these two groups in a reliable or valid way?
A. If the question is about any statistical technique, and that's right, if you are drawing -- trying to draw a conclusion about the exposed and what effect that would -- what effect exposure had on them relative to being controlled, and if you have an enormous enough control group so there are control people that look like those treated group, then you can use those control people. So let's suppose they are two standard deviations apart, huge, but you have

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10,000 times as many control people as you do treated people. It's possible that the tail of that distribution of control people will overlap in that region.

Q. Okay.

A. Then you can make comparisons, but due to a linear regression adjustment would take in that case these 200 exposed and these 8,000 control and throw them all in the computer program and push a button, and in such a case the linear regression adjustment, the slopes of those lines would be completely determined by the control people because there are 8,000 of them relative to the 200 treated, and then you would be sending straight lines down into a region where it's irrelevant, so it -- I'm trying to be helpful in answering your question.

Q. All right. I think I understand --

A. Okay.

Q. -- the main point you are trying to make. Let me ask you again about the Danish data set you were talking about.

A. Yes.

Q. I just wanted to make sure I did understand whether or not that data set met your test for being benign or not.

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A. No, it did not.
Q. What part didn't it meet?
A. Well the difference in means, I believe, was -- was greater than a half.
Q. Well that's not part of the test for benign, is it, or am I misreading this?
A. Unless the situation -- That's right, the benign. Okay. So first of all, it doesn't even meet the first part.
Q. Which is?
A. Which is, must be less than half a standard deviation. It doesn't meet that part. But benign has three conditions, that's right. The description of background characteristics are nearly symmetric. Do I remember whether it's symmetric or not? I think not. I think it was not symmetric. Distribution of the background have the same variance? No, they did not. The sample size the same? No, one was 8,000, the other 200. So, there was no way in the world I was going to do linear regression in this setting, yet doing something else, which is this matching idea, could -- could work because one group was so much bigger than the other that you could actually make some inferences about the -- for the exposed people for that group of people -- using that group

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of controls who were down in the same region of this factor space, the space of background variables, as the exposed.

Q. I think sometimes you referred to this larger group, sometimes referring to the tail or as a reservoir of people?

A. Yeah, a control reservoir, that's right.

Q. In situations where you don't have this large control reservoir so that your two groups are in at least in the same order of magnitude of the same size --

A. Right.

Q. -- is there some level where the different -- above which the difference in the means of the propensity scores gets to be where you can't make any valid or reliable statistical comparison between the two groups?

A. What's a hard-and-fast rule on that? I don't think there is a really hard-and-fast rule but anything approaching a standard deviation is pretty shaky because you are relying -- the groups are the -- Let me stop.

If you want to restrict inferences to the region where the two groups overlap, that's one possibility, so let's suppose they are far apart so, you know --

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it's an age -- let's do the age example; okay? The exposed are between 18 and 40 and the unexposed, the control, are between 30 and 60, and the standard deviation in a half difference in the means of the groups, that's substantial. I can't compare all of one group with all of another group. It's just too much extrapolation involved. But if I look at between ages 30 and 40, I find both smokers, exposed and unexposed, the control people there, so I'm going to say in this little example that let me talk about the effect of exposure but only on those people who are age 30 to 40. I have a shot at doing that because there is overlap with that subgroup of people. But if I want to say what's the effect of exposure for the people who are between 18 and 25, there are no control people anywhere near there. It's all fantasy built on some model that you just don't -- can't have any faith in extrapolating that far. See what I'm saying? Let's suppose we have people 18 to 40 who were exposed to, would be military people exposed to, I don't know --

Q. I think I understand.

A. They overlap. You can't -- It's like the sex thing again, they are all males who are smokers and all females that are nonsmokers, what can you say

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after adjusting for sex? There is a whole collection of better techniques than linear regression which sometimes include linear regression as a component. There is nothing wrong with linear regression locally, making local adjustments, within cells where people are relatively compatible and where the techniques include better modeling techniques, they include subclassifying, they include discarding some people who grew out of the range of the other group and restricting inferences to a smaller group. I've been writing about this stuff for 30 years so there is a lot of stuff I've written about.

Q. And to say.

A. Lot of stuff to say about it, yeah. And a lot of paper -- I don't know how many pages -- on specifically doing that better. And frankly, it's frustrating to read something like this report that is on a very serious issue, a very serious problem that's completely unaware of more than a quarter century of literature that goes back to before me, goes back to Cochran and his writing in the literature, including in the context of some smoking examples. It's sort of frustrating.

Q. Have you found other articles in the reported literature or other studies being done by people that

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frustrates you as well in the sense people are going regression analyses on comparing two different groups without having something like a propensity score analysis in the first place?

A. Sure. I hope I'm making inroads into those areas. I think I am in some fields, with other people as well, not just me.

Q. I take it that continues today, in 1998, there are still studies coming out or articles being written that use regression analysis, linear regression analysis comparing two groups without having verified that the propensity scores are -- meet your criteria and that the groups are close enough to be compared?

A. Right. I want to be clear that's not just -- I think propensity scores is a very efficient way of looking at the overlap in distribution. If someone did a careful job of looking for the overlap in distribution without using propensity scores, that's fine.

Q. You are not saying that's the only way to check for the distributions?

A. The critical issue is the overlap in distributions and this propensity score technology appears to be a very effective way of exploring that

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and being a component of a successful adjustment for background variables, if it's possible.

Q. And my question is whether people are writing articles, whether they are using propensity scores or some other means of checking the distributions of the two groups to compare. I understand your answer to be yes, you see them happening and yes, they are frustrating, to you anyway?

A. Yes. I wish that better statistics, better science was being done than is being done.

Q. Do I also understand that, you know, if the two groups that are being compared are fairly similar in size and you are not willing to restrict your analysis to a fairly small subset of one of those groups that happens to overlap the other, that if the difference in the means of the propensity scores is about a standard deviation or more, that in your opinion really no statistical means are available to compare those two groups?

A. Not necessarily none but linear regression certainly cannot be relied on, and if you tried to do a more robust, in the statistical sense a technical term, robust, then the answer of that will not be completely wrong with minor deviations in the assumption, so robust to model deviations. What

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would happen is, the standard errors, the honest confidence errors, will grow extremely large if you are in a situation like that. So that will be reflected, the real standard errors will reflect the fact that you can't say much when there is not much overlap whereas the standard errors from regression analysis are not honest that way.

Q. So then I guess I should understand that even if you have two groups that are roughly the same size and there are difference in the means of propensity scores is even more than the standard deviation, there may be ways to validly and reliably compare those groups other than linear regression?

A. Correct. There may be ways, depending on the actual sample size and how much overlap there is, it's possible to do something, but whatever it will be, whatever you are doing that's valid will yield much larger standard errors in general than would be the answer based on some assumption of straight parallel lines, which underlies almost all these kinds of models.

Q. If we look at the NMES data set that you performed your propensity score analysis on, which is the one that includes the imputations from NMES that includes the imputations with Drs. Zeger, Wyant and

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Miller, I take it it's your opinion that the difference in the means of the propensity scores for the smokers and nonsmokers was too great to use linear regression to compare those two groups. Is that right?

A. That in combination with the -- with the variance ratios, yes, that's my opinion, and also as stated by Cochran as his opinion from the tables that we did.

Q. Is it your opinion that other statistical techniques could be used to compare the smokers and the nonsmokers in that NMES data set in a way that would yield what you consider to be reliable and valid results?

A. It's possible that -- that there still is enough overlap in the distributions despite this large initial differences in bias and variance ratios, that we could take a look -- take a look at that to see what those other more robust methods would lead to for adjusting for background variables. It might be that's impossible. I don't know, I haven't done that next step in the analysis to actually try to do such an adjustment.

Q. So sometimes it's possible and sometimes it's just not possible to make that comparison by any

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statistical technique; is that correct?
A. That's correct. If the -- Again it's the male, all smokers are male, all the --
Q. Not just in that extreme examples that you have given but in a real data set like the NMES data set where there is certainly some overlap between smokers and nonsmokers, certainly their ages overlap to a large extent there, their genders overlap to a large extent and so on?
A. Right. You can have an overlap in each individual variable and have no overlap in the other direction. I can draw a picture if you would like to see.
Q. No, I understand what you are saying.
A. Okay. They can -- It's a combination.
Q. Right.
A. It's the --
Q. If you have 10 different categories, there may be no smokers who meet, you know, the same --
A. Right.
Q. -- set of all 10 of those criteria and there are smokers that do meet all 10 of those criteria, so if you want to use all 10, you can't compare any smokers to nonsmokers because there aren't any that meet all 10 criteria?

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A. Correct, that's correct. That description, though, would imply I really care about the 10-way interactions. I don't care about that. I'm not going to hold this analysis to that standard. That's too hard. But I would want to hold it to a standard at least for the main effects and two-way interactions. So if you have 10 variables, I would like in each two-way table to have overlap. I will not require overlap in all, in each cell, that you have both smokers and nonsmokers, because that would be a standard that would be too hard for any observational study to meet and would be just silly requiring that here. But I am going to hold it to a standard that says it should be -- there should be some overlap at least in the main effects and some two-way interactions that people regard as important.

Q. Is it important to look at all the two-way interactions among the variables or just certain ones?

A. For things like that, I would rely on someone else's opinion about whether these things interact, whether age interacts with sex, old males are different from old females. I suspect that's true but I'm not a doc so I want someone else to tell me

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whether that's important. In some of these analyses I have seen that put an age and age squared, because there tends to be not a linear effect on healthcare costs, even on a log scale, nothing much happens between some age and 50, 60, and then things start taking off, so that's a non-linear effect. Is that non-linear effect different for men and women? Yes, probably. Aging is different for men and women so you want an age by sex interaction. How about for pre- and post-menopausal women? Yeah. Is there difference in age and eating habits? I don't know. Age and exercise? I don't know. I'm not on top of that literature. The selection of which of those variables should be in there, the selection of which variables should be in there should be made by someone other than me, maybe with some advice from me what the consequence of adjusting for that are or how the two groups differ in those background characteristics, but the particular selection of which variables to control for and which functions, like the squares, should be made by someone else.

Q. Well in the Table 4 of your supplemental report --

A. Uh-huh.

Q. -- called the "Interaction Model" --

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A. Right.
Q. -- with the results, does that look at all the two-way interactions among the 25 background variables?
A. Yes, it does. That's right. And that was just to illustrate. I did three versions of it: One was just main effects, one was this interaction model which put a huge number of variables -- I guess 304 is what the title says -- and then this Table 3, which is something in between.
Q. Is it your testimony that these interactions, you would not want to necessarily examine all of them if you were actually trying to figure out whether that test is met by the NMES data set. You want to talk to some professionals in healthcare and so on to find out which are the important interactions?
A. Correct. And I would also end up probably including some variables in here.
Q. In here?
A. In the correct model, in the correct model. They are not even included in the interaction model. The interaction model only has two-way interactions. In your example of 10 variables, you had 10-way interactions in there. Now 10 way seemed too high to me but it certainly seems plausible to me there is an

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age, by sex, by exercise interaction. That's a three-way, and that's not in here. Perhaps it should be. If the amount of exercise has a different effect for old men than old women, than young men and young women, and that would be a three-way interaction. That's not in here. And if a health professional said that's important, there is an important difference of the -- it's important -- how much you exercise on your healthcare costs makes a difference in each of these four cells of old, young men and women, there should be three-way interaction in here and that's not in here, and that could create an even bigger difference. That's why --

These analyses are meant to really show that the main effects is to show just what they did, just what the Zeger team did, and that's the minimum amount of looking at the overlapping distribution. Table 2 and Table -- I'm sorry. Table 3 and 4 were just indications of other things that you could put in but in fact in the step-wise for the two-way interactions and all the two-way interactions, but in fact the right thing to do would be to talk to healthcare officials to find out which two ways are important and the other, which three ways should be in there as well.

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Q. So the one model you would definitely keep after talking to a healthcare professional, so on and so forth, I take it would be the main effects model?
A. I wouldn't say one I'd keep but that just shows how -- that shows how bad the adjustments that the Zeger team did are, even if you only were worried about the variables that are in the Zeger team model in their sample form.
Q. My question, if you were looking at this NMES data set, asking yourself can you use linear regression to compare them or not, am I understanding correctly that you don't need to talk to healthcare officials or anyone else --
A. Correct.
Q. -- to do the main effects model?
A. Correct. They select -- That's their model. That's exactly what they did and this says even if all the data in NMES imputed are correct, that these are the only variables you want to adjust for, then you cannot trust what they did.
Q. And the Tables 3 and 4, the interaction models, the regular and step-wise models, are things that you would want to consult with a healthcare professional or some other professionals first before actually performing that type of analysis to determine whether

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the NMES data set will allow these kinds of comparisons be made on a linear regression basis?

MR. BIERSTEKER: Excuse me just a minute. I object to the form of the question.

A. If these two tables, Table 3 and 4, are relevant to the question as follows. Here are the background variables we want to adjust for, but let's suppose instead of just their main effects we want to worry about other possible functions in them. Two simple kinds of functions are all interactions and another would be a step-wise, stepping through and taking the ones that appear to be important, here's the kinds of results you would get. Now in fact a healthcare expert might say, well, really we should be including more variables than this that reflect certain three-way interactions.

Q. But we don't know what --

You don't know, anyway, Professor Rubin, what a healthcare professional is going to say because you haven't asked one yet; true?

A. Correct. But apparently they think the variables they put in their main effects model are relevant because that's what they did.

Q. But we don't know whether healthcare professionals would think that all the two-way

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interactions that flowed from those 25 variables are important; right?

A. We don't know that but we don't know whether they understand that the model they fit only adjusts for these main effects and does not do what you described, which is adjusting for each cell. It's not doing that. If they want to do that, they have to put in 25-way interactions because it has to have a parameter for each one of those cells. You know what I'm saying. They product all these variables out. But the way the models are described often in a linear regression is deceptive. I put in age in the model, yeah. I put in sex, yeah. I put in exercise, yeah. And I've adjusted for age, sex, exercise just as if I formed those three-way -- three-way table. That's the way it's often described. I'm saying that's wrong. It doesn't do that. All it does is adjust for these marginal effects on those variables. It doesn't do what you were describing, which was put them in the same cell. So that's what these other tables are doing, is trying to look -- let's suppose you want them in the same cell but just the two-way faces, not really in the same cell, because I think that's what people think a linear model doing, doing more than what it does.

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Q. But if we assume you described to healthcare officials what your models really do, they may tell you that they want to examine some of these two-way interactions and not others. We don't know what they are going to tell you; right?

A. Correct.

Q. And so the propensity score analysis that we would do in the interaction and step-wise interaction models could be significantly different than what's shown here in Tables 3 or 4 of your supplemental report?

A. Significantly different, I doubt that. Could show bigger differences, that's true.

Q. Could show smaller differences; true?

A. We know it's bounded from below by the main effects model, so it's at least that different. It could only get more different as you add more variables.

Q. Is it the difference in the means that's bounded from below or is it the difference in the ratio of variances, or what's bounded?

A. It's really the -- the means in the sense -- See, how to say this? If I have a group and they differ on some set of variables, like the main effects model, and now I want to bring in another

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variable, and let's suppose that variable, after adjusting for these ones that are already in there, has exactly the same distribution for smokers or nonsmokers, then bringing that variable in will not move these groups farther apart. They have the same distribution, both groups. But let's suppose it has a difference, even after adjusting for these other variables. All it can do is make it bigger. All it can do is make them farther apart if analyzed the right way.

Q. If we compare -- The letter capital B that you use in these Tables 2, 3 and 4 stands for the difference in the means of the propensity scores of the two groups set forth in how many standard deviations away they are; correct?

A. Correct.

Q. If we look at the main effects model, Table 2, and we compare the figures under the B column there with those under Table 4, the interaction model --

A. Right.

Q. -- do we in fact see that for at least the first -- really for every group, all six age/gender groups, that B is smaller in the interaction model than in the main effects model?

A. What's -- what's going on here is, there is some

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confounding with the ratio variances that's taking place so that -- let's look, for example, at the first -- the first row for females. See, B is the number of standard deviations and as you bring in more variables, the ratio, the standard deviation can change within a group, and so B is -- remember in your example how we calibrated by dividing by the number of standard deviations.
Q. Right.
A. As you bring in more variables, the standardization itself of this propensity score can change the ratio of standardization can change. The thing you are normalizing downstairs can change. For example, in the first row you see that's right, B goes down, but see how the ratio of standardization is completely different, goes from 71 to 21?
Q. Seventy-one is a better ratio for your purposes in terms of having what you call --
A. Benign.
Q. -- a propensity score that is consistent with linear regression; correct?
A. Correct, correct.
Q. Let's look at the second row, though, the females 35 to 64.
A. Right.

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Q. Isn't it true that -- that not only do their B go down, which makes them more consistent with linear regression, but also that their ratio of the variances gets closer to Y, which also makes it more amenable to the linear regression?

A. That's right. And then I think if you look to see what's going on in the direction that's called, technically, orthogonal to the propensity score, which has to do with the right-hand part of the table, things are looking pretty severely different there in terms of the -- see, there is no mean bias in these other directions. I've taken care of that by taking out the -- by looking at residuals off the propensity score, and what you see going on there, there are a lot of directions in a geometric sense directions, combinations of variables where the variance ratios are severely different. So I think I was fairly careful in my answers. I was talking about things would get worse if you add more variables. I cannot say the B will get bigger or R will get bigger. I hope I was careful. I said the distribution would become farther apart and the way they get farther apart can turn around different ways, so sometimes will be reflected in the bias. Often in very simple cases that would generally be

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true where the groups are symmetric and have the same variates, but it can be reflected in other ways in these directions or orthogonal to the propensity score.

Q. And that's what you call the variance of the residuals remaining after adjusting for propensity scores?

A. Yes, that's correct. Because these things, these residuals, after adjusting for propensity scores, are themselves particular combinations of the variables that were in the model and if it turns out that the outcome, the expenditures, is a function of a variable in that direction, then although the variable has the same means, the variances are entirely different and you get a position where linear extrapolation can be -- linear modeling, linear extrapolation is very non-robust, even if the means are the same, because they can be sensitive to a little bit of curvature in their response surface that rises at one end, does not rise at the other end.

Q. While looking at Table 2, the main effects model results, in that last grouping on the right called R for covariates after adjustment, that's what we have been talking about as the orthogonal?

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A. Right, orthogonal. That means after I've taken out the propensity score, in a regression sense, in some sense it's regression sense, just looking at the distribution of these variables after having pulled out the propensity score, so all of these residuals which is described in the table have the same mean effectively in the smoking and nonsmoking groups but their distribution is still different and how they differ is -- I'm just summarizing -- is differing by their variance ratio.

Q. If we look at that table, there is five columns to that part of the table?

A. Correct.

Q. And I take it the middle column between a ratio of four-fifths and ratio of five-fourths is what you would consider to be --

A. Pretty benign.

Q. -- good?

A. Yeah, that's right.

Q. Where did you -- How did you determine that four-fifths should be the lower bound of that range and five-fourths should be the upper bound of that range?

A. Not based on anything that has been published so far. The stuff that's been published concerns the

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less than half, greater than two. The four-fifths and five-fourths is based on a mental combination of various things I've seen over the years. It's not -- I couldn't point you to any particular place where the four-fifths and five-fourths came out.

Q. But in the published literature there is information about this R for the covariates after adjustment being problematic if it's lower than a half or greater than two; is that correct?

A. Right. Because -- But you won't find the literature after adjusting for covariates. What that shows, if you have a background variable, which each of these covariates after adjustment is, is just a combination of background variable, if you have a background variable, even for which there is very little bias in the means, almost no bias in the mean but the variance ratio is bad, like greater than half or less than half, are you in trouble using linear regression? The answer is yes.

Q. Greater than half are --

A. Greater than half or greater than two, less than half -- it's just symmetric. It's a ratio, the same thing, one goes one way, the other goes the other way.

Q. The variance ratios are problematic when they

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are less than half or greater than two?
A. Right. I apologize if I didn't say that.
Q. If we look at Table 2, the table for R for covariates after adjustment, if you look at the ones -- there are none greater than two, is that correct, on this table?
A. Correct.
Q. If you look at the ones that are less than or equal to a half --
A. Yes.
Q. -- just take the first row, females 19 to 34, there was one such covariate; right?
A. Right.
Q. Do you know how much less than a half its ratio was?
A. No, I don't. I mean I -- I think at one time I was thinking about doing this part of the table in a way by actually showing percentiles of those Rs, those ratios, that would give specific numbers, and I think I concluded, and I don't remember why, that displaying it this way was much more transparent, instead of having all these point seven three twos and point oh two ones. I don't remember exactly why.
Q. But anyway, you don't know how much less than a

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half that one particular covariate was?
A. No.
Q. Assuming it was a ratio?
A. Yes.
Q. Do you know what covariate that was, which variable it was?
A. No, but I could find out. But remember, after the adjustment, it's now a combination of lots of covariates, so it could be something like age adjusting for the propensity scores, could have all kinds of other things built into it.
Q. Is there anything that you provided to the plaintiffs in this case that would allow them to determine what that variable or relationships of variables is that represents the one that had an R for covariates after adjustment after less than a half?
A. If Raghunathan provided the disk that did the analysis, then yeah, you can look at that and figure it out.
Q. Is that a disk that was provided with the supplemental report or disk that was provided back when we received exhibit --
A. This would be supplemental report. I didn't do any of these covariates after adjustment until I did

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this report, so that would be with the supplemental report.

I did consider other ways of doing this right-hand part of the table as well using things that I thought would be just be more confusing but in some ways would be -- not only were sophisticated but a little bit sharper, having to do with something you don't want -- I don't think you want to hear about it. I'll talk about it if you want to. I think this is -- I regard this as a relatively transparent way to describe what's going on with these other variables, "other" meaning other than the propensity score.

Q. And if you have, you know, one covariate after adjustment that falls outside of what you consider to be a reasonable range, does it make sense to examine what that covariate is and ask healthcare professionals, or whatever if you are looking at smokers or nonsmokers, whether that factor really is something that was considered to be a significant factor in looking at the relationship between smoking and healthcare costs?

A. Yes, I think that makes sense.

Q. So you would want to know what they were, the covariates that fell outside of the range and examine

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those?
A. That's not a bad idea. It's also true, though, that the selection of covariates was made by the Zeger team, presumably after some thought, and they wanted to adjust for these, these variables, and so if after doing the analysis you say, woops, that one looks like trouble, I now want to pretend like I never wanted to do that, that's not quite fair.
Q. Well this is doing it in an intellectually honest way. You may have included variates that you thought were marginal initially and one turns out to have -- falls outside the range on your test for R for covariates after adjustment. Then it may be that that variable really is not thought to be particularly well associated with smoking and not really part of any causal pathway, or associative pathway for that matter, but was included anyway for whatever reason, because somebody else included it.
A. Yes. It's hard to do that intellectually in an honest way but with that adjective, sure, that's okay to do. But everybody knows if you have an analysis you want to get through to the end and if there is something standing in the way can I find a reason, can I rationalize why that shouldn't have been there after having decided it should be there, now it's

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creating problems, can I find a reason. I can write a paragraph why I should take it out.

Q. Would it also be something that would be appropriate to, if you do take it out, make it explicit that's what you did and explain what the analysis that led to that decision was?

A. Absolutely. The analysis, not only statistical analysis, but intellectual analysis that led you to take that out afterwards. It's better to do that a priori, like exclude 10-way interactions, exclude 8-way interactions, exclude two-way interactions that you don't think have any medical consequence so that you don't have to make any kind of intellectual argument later and say I really am that honest. It's better to do that in advance than after.

But you're correct, that one could look at that and say that's a problem, can I now make an argument for why I do not have to adjust for that even though I said I should at the beginning.

Q. All right. We can turn back to the first page of your supplemental report. Under the heading Roman numeral I, Propensity Score Methods, near the bottom of the first paragraph you are saying without such an analysis of these distributions of smokers and nonsmokers, one cannot have any confidence in the

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Zeger report's regressions' ability to adjust reliably for background differences between smokers and nonsmokers; is that correct?

A. Correct.

Q. Are you saying that it's impossible for the Zeger report regressions to have produced valid results or proper results?

A. Am I saying it's impossible that the numbers that they get are the numbers that you would get having done something -- something that is correct? Is it impossible those two numbers are the same?

Q. Yes.

A. No, I'm not saying it's impossible those two numbers are the same. I'm saying the process is not correct. My analogy is that you are in a restaurant and there is somebody completely drunk and he wants to drive home, see. Is it impossible that he will get home safely without having an accident? Here's someone else who is sober and he is going to drive home, you say is it impossible that they will both get home at the same time and without having an accident? No, it's not impossible, but certainly I would have more faith in the sober person reliably driving home than the person who can't even stand getting behind the wheel and flailing his way home.

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It's not impossible he will get there but I don't have any confidence.

Q. That's what I'm trying to understand. In your opinion you are saying the methods they used aren't correct in your opinion but I take I you can't give an opinion as to whether the results they obtained are correct results or incorrect results?

A. I can give you an opinion that the same analogy with the drunkard, that I think it would be very unlikely the drunkard would get home safely. I think it's quite unlikely looking at these results that the answers are correct and it's the same analogy.

Q. By "these results" you mean propensity --

A. Yeah, the overlap distribution of these background variables.

Q. Then the next paragraph, bottom of page 1, you say, "The statistical literature warns that regression analysis cannot reliably adjust for differences in background characteristics when there are substantial differences in the distribution of the background variables in the two groups"; correct?

A. Correct.

Q. Now can you identify any statistical literature that sets forth that warning other than literature that you've authored or was authored by your thesis

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advisor, I guess you call him, Professor Cochran, or one of your graduate students?

A. That warns about regression adjustment? Yeah, there are other literature. Can I recall them right now? There is some literature in economics that worry about model specification, the whole collection -- Let me be clear. There is a specific problem of using linear regression to adjust for differences like this in an observational study. There is a broader question of relying on linear modeling to draw conclusions, and for that broader question there is enormous literature of worrying about the non-robustness of linear models just in general in many fields. It's enormous in statistics. When focusing on the more specific question of using those kinds of linear models to make adjustments in observational studies, the literature is smaller, and then you have to look then in subfields where people are doing this kind of analysis. What I mean by "this kind of analysis," taking an observational study, not random, observational studies, trying to compare exposed, unexposed, adjusting for a collection of background variables.

Q. That was the --

A. Context?

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Q. -- context in which my question was meant, Professor Rubin, in using statistical -- using regression analysis, linear regression, to adjust the background variables.

A. There is this book that I referred to I believe in here that was -- I think Hauck was one of the editors, von Dam. Where did I have this?

Q. The bottom of page 4 of your report?

A. Right, Anderson, Auquier, Hauck, Oakes, Vandaele & Weisberg, Statistical Methods for Comparative Studies, that has that kind of information in it.

Q. What kind of information does it have?

A. Information that linear modeling adjusting can be unreliable even on log-linear -- linear-log or logit model as well as linear assumption. I referred to that. There are articles in -- recent articles in economics literature where that arises as well.

There is a growing awareness of the problem and, as you probably realize, the advent of computers made it very easy to dump data in, push a button and getting something out. It's a very fast way of getting publications. And they are -- the dangers -- although the dangers of doing analyses like that have been known for 30 years, there is still a temptation to do an analysis that does it because the answers

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come out right away.
Q. Does the Anderson reference at the bottom of page 4 of your report describe propensity scores as you discussed them in your supplemental report?
A. Not exactly as I described it but warns about the lack of overlap and distribution, what it can do. A propensity score paper wasn't published until 1983, which is after this book was published, so if they were -- if they talk about those sorts of things, which is sometimes called discriminate matching, or above that it's -- they talk about overlap and distribution issues, but they certainly do not use propensity score jargon because it didn't exist then.
Q. Can you identify for me today, from your memory, obviously, any other article or book besides the Anderson reference at the bottom of page 4 where someone other than you or Professor Cochran or one of your graduate students describes this problem of using any type of linear regression when the background -- when the -- without comparing the distribution of the background variables first?
A. I believe there is some in economics, there is certainly one that was a thesis that came out a couple, three years ago in the department of

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economics at Harvard. I think the article is in its final stages of being accepted for publication in the Journal of the American Statistical Association by a couple former graduate students at Harvard.

Q. Now were they in the department of statistics in Harvard or some other --

A. Economics.

Q. Economics. So they weren't students of yours, then.

A. No. I talked to them but that's -- they weren't mine.

I'm sure there must be a variety of papers that aren't coming to mind but I can certainly talk to epidemiologists who I would regard as being aware of this issue of modeling assumptions and sensitivity to modeling assumptions, because it motivates all sorts of techniques that try to get away from explicit modeling assumptions. Some of the stuff I like, some of the stuff I don't like, but they try to be more robust. Sometimes it goes under general estimating equations approach in doing these kinds of adjustments, trying to get away from reliance on specific functional and form specifications. In economics, it's often known as functional form specifications and how you have to worry about that.

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I could try to do a much better job of accumulating some references on that.

You say and my students, I was thinking Paul Rosenbaum but I guess he doesn't count despite that he has been a full professor at Wharton for 10 years, he doesn't count.

Q. He certainly was a student of yours. We don't let people forget that, do we?

A. Evidently not. He has to be ashamed of it, that's even worse. I'll have to call him tonight and tell him.

Q. So as far as your memory sitting here today, there is nothing other than -- than the Anderson reference at the bottom of page --

A. There is a number of vague things; I can't remember the specific references.

Q. I understand. But you can't identify an article or book?

A. Right.

Q. At the moment.

A. It's like asking did you know driving drunk is bad for you? Yes. Can you give me a reference for that? How many references are there? I don't know. There are lots of, there must be many of them that make this point because it is a really rather obvious

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point, and I'm not being very good at recovering them.

Q. This point isn't as obvious to most people as driving drunk is dangerous.

A. Correct.

Q. Turn to page 2 of your report, Professor Rubin, and at the bottom you have a quote from an article or book that Professor Cochran wrote in 1965; is that correct?

A. Yes, I think that's an article; correct.

Q. Now in there he is talking about whether you can trust regression analysis. In fact, regression adjustment is in square brackets in that quote you put in?

A. Correct.

Q. What did you mean by putting that in brackets there?

A. He doesn't say that. He says none of the methods, but the article is entitled "Analysis of Covariance: Its Nature and Uses." Analysis of covariance is another name, older name for the kinds of regression adjustments that are done here. He refers to the fact that there is this background variable X and there is an outcome variable Y, and the way in the old days, pre-computer days, you did

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this adjustment was to do what's called an analysis of covariance, which is basically an analysis of X times Y, of the covariance between X and Y. It's an old name, I think it's due Fisher. So in an experiment or study where there is just one X and one Y, the old name that goes back to that, to probably the '30s or '40s, is called analysis of covariance, the use of regression adjustment in an experiment to do exactly the kind of adjustment we talked about here is called analysis of covariance.

Q. So it's your understanding that, although Professor Cochran's book didn't require regression adjustment not being trusted to remove all the bias, that's what he meant based on the title and the general nature of the article?

A. Correct, the title of the article, and my memory is that in fact this may have appeared in the special issue of biometrics devoted to the analysis of covariance. I'm not sure about that but I think so.

Q. Now you never know for sure as a statistician, do you, that any statistical method has removed all the bias?

A. That's correct.

Q. In fact, you don't -- you never know for sure as a statistician that any statistical method has

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removed any of the bias; correct?
A. That I don't understand.
Q. I mean, how do you know for sure that -- that the statistical method has removed bias from the problem if you don't really know what the truth is?
A. Okay. Let's go back to your example. In exhibit -- on the floor over there.
Q. Okay. Exhibit 3551.
A. Good. Now here there is initial bias between smokers and nonsmokers due to gender; correct? That's the way we drew it. There is initial bias to the gender and in fact smokers are more heavily male and nonsmokers are heavily female.
Q. If that's what you mean by bias, yes.
A. That's the only thing I understand. If you have another meaning, you can tell me.
Q. I just want to use your meaning. That's fine.
A. Okay. There is an initial bias due to gender, so we compare them within males and within females. Have we removed the bias due to gender? Yes, we have removed the bias due to gender by comparing within males and females. You can no longer claim by comparison between smokers and nonsmokers within males is due to the bias in the gender distribution. I'm only looking at males. So you have removed bias

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there and you are confident you removed bias due to gender.

THE WITNESS: Could we take a break?

(Recess taken from 4:32 to 4:45 p.m.)

BY MR. LOVE:

Q. Professor Rubin, looking at page 3 of your supplemental report.

A. Okay.

Q. In the first full paragraph, starts out with the word "In particular."

A. Yes.

Q. You say there are three basic distribution conditions that must be met simultaneously for regression adjustment to be trustworthy; is that right?

A. Right.

Q. And then you say if any of those conditions is not satisfied, then regression adjustment can't be used; is that correct?

A. Well I said cannot be -- cannot be used to get reliable and trustworthy answers. I'm not trying to be precise. It can be used, meaning somebody did it. They pushed a button and did it so you can do it, but can you believe what comes out.

Q. You say in your opinion you can't believe what

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comes out if it doesn't satisfy all three of those conditions?

A. Basically, right.

Q. And therefore when we looked at, for instance, Table 2, the main effects model back around page 11 of your report, I guess --

A. Right.

Q. -- no matter what figures we may have seen on the R column or for the five R for covariates after adjustments columns, if the B column shows figures greater than a half a standard deviation, it's your opinion that linear regression should not be used; is that right?

MR. BIERSTEKER: Object to the form.

A. Should not be used in the way it's used in the simple blind adjustments without looking. There are ways to use linear regression in combination with other things but I'm referring to -- what I'm referring to here is the way the linear regression models are done in this report.

Q. Simply using a probit linear regression model or logit linear regression model without doing some comparison of subgroups or whatever?

A. Or linear log for the expenditure amounts, that sort of modeling exercise.

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Q. All those things would be improper in your opinion?
A. Improper to do it that way and stop there; correct. Let me -- To clarify just a little bit, if you are fooling around for a little data set for classroom exercise to illustrate a point, you could do something like that, wouldn't bother me. But if you are trying to get an answer that you believe is important, then you should exert effort to try to see that it is -- that you are doing something that's reliable.
Q. And the first condition that needs to be satisfied that we talked about earlier was the difference in the means of the propensity scores being small?
A. Correct.
Q. When expressed in terms of how many standard deviations away they are.
A. Correct. Let me just hear the question again, make sure --
Q. The first --
A. It's getting towards the end of the day and I'm afraid I'm not attending with the same rigor I was earlier.
Q. The first distributional condition that you say

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must be simultaneously met to use linear regression of any type in the way that Professor Zeger, Wyant and Miller did --

A. And trust the results.

Q. -- is that the difference in the means of the propensity scores between the two groups being compared must be small.

A. Correct.

Q. The second --

A. Unless --

Q. -- condition.

A. Yes, okay.

Q. Subject to this benign situation.

A. Right.

Q. The second -- First of all, have you examined the NMES data to determine whether or not it fits the benign conditions?

A. Yes. We can see going back to that main effects model that it's bigger than a half but the variance ratios are in that column, are -- many of them are right around a half, which is very small. One is a fifth and one is three quarters but with a large bias, so I consider all those cases to be troublesome, all six.

Q. Which of the A, B and C benign tests are not

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being met there?
A. I'm not doing it in a -- in a test way. I'm saying these are indications of problems so if you -- if you look at everything except the first row, all the variance ratios are bumping around a half, which I consider troublesome, and the one that's point six five, where the variance ratio is point six five, the bias is extremely -- exceedingly large, is point eight eight, so I'm putting those two things together and saying that's really disturbing. In the first row, the same sort of thing, the variance ratio isn't as bad as the other cases, it's point seven one, but the bias is really quite substantial, so --
Q. The "bias" meaning?
A. The B, the B --
Q. Okay.
A. -- is quite a bit bigger than half and the variance ratio is not below a half, is less than three quarters, so I'm -- I'm worried there. The second one, the bias is a half consideration, barely under half, but the variance ratio is a half so I'm worried there.
Q. Professor Rubin, let me ask you this, then. Are these conditions under which the situation is benign that you list, and you list an A, B and C condition,

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is that something different than just combining the first of your three distributional conditions, being the means of the propensity scores being -- difference in the means being small and the second one, the ratio of the variances being close to one? What I hear you saying is that if you can't meet the first condition, number one here on page 3 of your report --

A. Right.

Q. -- or if you can't meet the second condition, number two, on the bottom of page 3 --

A. Right.

Q. -- if you miss one of them, either one or two, then it will never be what you call benign; is that right?

A. Correct. But I'm saying that in addition to having -- having two, the ratio variances, you need A and C to be benign, you need -- the distributions have to be nearly symmetric in both groups.

Q. And --

A. And you have to have the -- okay.

Q. It's even more restricting than?

(Interruption by the reporter.)

Q. It's an -- In order to be benign, it's even more restrictive than failing to satisfy both one and two

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simultaneously?
A.   Exactly.
Q.   And the third distributional condition that you say must be satisfied in order to use the linear regressions in the way that Professor Zeger, Wyant and Miller is the ratio of the variance of the residuals of the original covariates after adjusting for the propensity score must be close to one; correct?
A.   Correct.
Q.   Now is there any publication you can identify for me that sets forth those three tests?
A.   Not stated just that way. I wish that there were.
Q.   Is there any publication that, you know, single publication that sets forth those three tests in some other way?
A.   Well the -- Yes. In fact, the table that I refer to from this Cochran and Rubin paper in 1973 shows what can happen for a single covariate, for one variable, when you try to do a linear regression.
Q.   This is Table 1?
A.   I think so. Yes.
Q.   Table 1 on page 10 of your report?
A.   Correct, correct.

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Q. Now --
A. This is what happens for covariate, or one X variable.
Q. Can you explain to me how Table 1 shows that the difference in the means of the propensity scores must be less than half a standard deviation apart?
A. Okay.
Q. Here in Table 1.
A. Sure. The propensity score is just a particular kind of covariate. It's the same thing as if you made up an index that said you get one point if you are male, you get five points if you are sick, you get minus three points if you exercise a lot, make up an index like that, and that becomes a variable. Could have done it by putting down numbers like that and adding them up and you get a score for overall health, or could have done it analytically, "analytically" meaning by computing something, and the propensity score is one variable, a combination of all the pieces of information that went into it, a combination of 25 covariates, so that's one variable now. Propensity score is now one variable and think of it as one variable. Now go into this table and say, okay, let's suppose the bias is big on that one variable and let's say above, like a half or three

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quarters or one, and look to see what happens when the ratio of variances, that's the left-hand, first row is 2, where the ratio variance is a half, which is the bottom. How well does linear regression do at removing bias?

Q. Let me ask you --

A. It's terrible.

Q. -- ask you about this table.

A. Right.

Q. Is what we really should be concerned about is the figures inside the table or is the B value and the R values and the sort of axes of the table?

A. Okay. What we can -- we can calculate from our data are the B values and R values. We see those. So for example the propensity score is one variable, we can see what B is in the different -- In Table 2 I showed you various values of B in different cells and what the R value is, so these values we know from looking at the analysis of the data we have got. What you don't know is which of these functions, the Y, moderate, nonlinear, moderate, marked, we don't know what God is doing to us there. Only God knows that.

Q. That's to describe the true relationship in the world?

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A. Right, between Y, the outcome, expenditures or -- this is for continuous outcomes so it relates to log expenditures. Only God knows that. We don't know what it is. Moderate and marked refer to the fact that how much curvature it has relative to the straight line, and those are Cochran's words on moderate and marked, and so let's see what happens when we try to do linear regression adjustments where we may know where we are with respect to R and B but we don't know which of these four columns we are living with.

Q. Right.

A. Okay? So let's take -- if we look at -- if we know R is one and B is a quarter, well, gee, linear regression does very well. It's just about a hundred percent, maybe 1 percent overadjustment, slight. How about if R is one and B is a half? Gets to 2 percent. Now it's 2 percent of a bigger starting amount because B as a half is a bigger bias so it has a bigger bias than it did in case one but it's still not bad. When B is three quarters and the variance ratio is one, R is one, well 4 percent overcorrection for these conditions, but if you get 4 percent of a bigger bias to start with, that's good. In our case it would only be a few hundred million dollars,

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perhaps. B is one, then it still is sort of okay, although it doesn't get all of it, overcorrect. There is an 8 percent problem there. And we don't know which of those answers is right, whether it's a hundred percent or 101 percent, 102 percent or 108 percent, because all we know, we are living in the B equals, let's say, three quarters and the R equals one, so that's what that would say.

Are we ever there with these data? Well let's look another Table 2. Where are we? We are living in a world where R is about a half, sometimes a fifth, sometimes three quarters and B is over a half. So where are we living? We are living in the bottom row. And where are we living in the bottom row? We are living at about the B equals three quarters and R equals a half, or maybe B equals a half and R equals a half. That's where we are living when we look at these values. We are living in this region. And you look in that region and you see what is the linear regression adjustment doing. In some cases, to use Cochran's words, it's widely erratic. Sometimes it overcorrects, sometimes it undercorrects. It can't be relied on and is varying with something that they are never worrying about, these modest -- moderate nonlinears or marked

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nonlinears which they never look to see whether they were there.

Q. All right. Just so I understand the bench marks that you have set forth here --

A. Right.

Q. -- taking a simpler example, if we just had a case where R equaled one and B equaled three quarters, under two of your assumptions about how the world really works the linear regression could produce pretty good results; isn't that right?

A. Pretty good, although 4 percent --

Q. Well under the moderate it's only one percent; right?

A. One percent would be pretty good and 4 percent would be okay but there is a bigger -- when B equals three quarters there is a bigger initial bias to start with, so it's 4 percent of a bigger number than lower down in the table.

Q. And you already told us that.

A. Okay.

Q. If R equals one and B equals one and you are under one of these moderate nonlinearity situations, it's only a 2 percent, again it's 2 percent of a bigger number you will tell me but it's still not terrible?

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A. That's right. It's the linear adjustment is trying to do the right thing and comes pretty close, and so I would consider such a situation relatively benign. There is an implicit assumption here, by the way, that the number of observations in both groups is the same.

Q. Okay. The -- you have not -- You told us about these sort of four functions that could possibly try to describe the real world.

A. Right.

Q. And do you have any opinion as to which of those four functions, if any, fairly describes the relationship between smoking and health or smoking and healthcare costs?

A. Absolutely no idea.

Q. Does it have to be one of those four?

A. Absolutely not. Could be much worse.

Q. Could be something entirely different?

A. Yeah, could be something entirely different.

Q. And if it's something entirely difference, we don't know how much overcorrection or undercorrection has taken place, do we?

A. Correct, that's right. Pretty good reason to see what condition you are in.

Q. Well you can look and see what condition you are

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in but if the real world is not anything of these things, what do you know from this table about what any model is doing?

A. Okay. What you know, these are trying to be representative of moderate and marked and nonlinearity, of lack of model fit. So if you are in a case the R equals one and B equals a quarter, B equals a half, you can feel pretty comfortable about using linear regression unless the truth is really way off linear, and depending upon the kind of data you have, you may be able to actually see the way off if you look at it, which would be a wise thing to do. However if you are in the let's say R equals two equals, B equals a quarter world, you are being told there you are really in trouble unless you really can tell me that you think the world is linear, pretty close to being linear so that you are down to moderate. And even if you are down in the moderate, you can still be in deep trouble. So, you better look very carefully and you better not rely on the linear regression adjustment to do the adjustment for you. It could go either way.

Q. If you have a situation where R equals a half and B equals one, you could have adjustments that are only 2 percent too much or 4 percent too little or

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you could have adjustments that are 40 percent too much or --

A. Correct.

Q. -- 13 percent too much?

A. And these are in a situation, in that case R equals a half and B equals one. I know I'm repeating myself but I want to emphasize that the initial bias is large, so 40 percent of the large numbers is large.

Q. But we might be in the 2 percent plus or 4 percent minus category, too, or some other --

A. Could be worse.

Q. Could be worse or could be better than that, could be 1 percent depending on the what the real world --

A. If the real world were linear, you took it all up.

Q. What is it -- Do you have an opinion as to whether the real world is linear or not with respect relationship between smoking and health or smoking and healthcare costs?

A. We know all models are wrong so it's not perfectly linear. The logit specifications, the probit specifications have linearity built into them that we know are wrong. We know that the log

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linearity of the expense fund is wrong. How wrong and whether it makes a difference, we don't know, but we don't want to rely on the linearity assumptions to do these kinds of adjustments. We want to do something that's more robust to the specification of the underlying function. A good method would be, try to clarify, would be if we did the same table, we had percent reductions bias using good method. What this would show in contrast would be throughout the table there would be numbers between 90 and 110 percent. That would be a good method.

Q. So 90 to a 110 would be a good --

A. And then even better would be now supplement it with regression adjustments across all versions of the table, and that's a paper actually wrote in '79 on match sampling which is referred to in doing the Danish study, and that's why we did it that way, because that's what tends to come out if you do it well, if you do the method, the same sort of setup as this. You find numbers that are -- give you some faith that what you are doing is not completely dependent upon something you have no idea about and you haven't even looked at. That's what a good method would be. This is not a good method, especially when you look at the data and you know you

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are in the R equals a half, B equals a quarter, B equals a half, B equals three quarters cells, it just --

Q. But since we don't know what the real-world distribution is, we don't really know what the percentage of overcorrection or undercorrection would be; right?

A. That's correct. But we do know what we have done cannot be trusted, it's completely unreliable. It is making up numbers.

Q. Well if the model is not right, it can't be trusted. If the model turns out to be right and actually reflects the real world quite reliably, you are going to get good results?

A. If the world is exactly linear, that's right. Tautologically, if the model is exactly right and you apply the model correctly, you will get an answer that agrees with the model, not the way they did it. They are missing data. This is a model missing data and other things.

Q. Now we talked earlier today about the two-way interaction part of the Table 4.

A. Correct.

Q. And the step-wise interactions that was Table 3.

A. Correct.

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Q. Can you give me any references to literature that say that those calculations need to be performed before you use linear regression in the way that Drs. Zeger, Wyant and Miller did?

A. Step-wise in all two-way interactions?

Q. Right.

A. No, I don't think I can say that has to be done. What has to be done, as I think I said before, you should decide a priori what the right -- strike "right" -- what the collection of background variables you want to adjust for and then decide which interactions among them you think are important and put them in and then try to see how far apart these distributions are having put all that in, so they decided, "they" being the Zeger team, decided the main effects model was adequate, so that's the fundamental place to start. And these other tables are just showing how the variance ratios, for example, orthogonal to the propensity score, can get extreme if somebody wanted to through two-way interactions and even -- or by step-wise kinds of things by including two-way interactions. I would not say that's something that one should always do, no.

Q. Now do you review articles that are submitted to

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various peer-review journals?
A. Yes.
Q. Have you ever reviewed an article written by someone other than one of your students, anyway, where the person uses linear regression and they have explicitly set forth their comparison of the distributions of the two groups and seen that it met the test, the bench marks that you have set forth and have gone through and done not only a main effects situation but an interaction effects situation as well?
A. So you are setting it up so the person has to have used propensity scores and done it the way I did it here, but I said I wouldn't even recommend doing the interaction step-wise. Is that really the question, though?
Q. That's not exactly step-wise but using propensity scores, not just in a main effects situation but also some kind of step-wise or interaction, whether it's two way or three way or certain variables are two way and other variables three way, whether it explicitly sets forth here is what I've done to be sure that I can use linear regression here.
MR. BIERSTEKER: Objection, I think that

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mischaracterizes the testimony.
A. Yeah, I think it does as well. You are saying have I ever seen somebody do something that I wouldn't recommend that they do. I've -- As I've said, the reason I was doing that is that here are the collection of variables that Zeger et al think should be adjusted for. Often there is the implication in people's minds that linear regression, just putting in main effects, adjusts for everything in a cell-by-cell basis the way you described it earlier, and this was just a way of illustrating the first step. If you really wanted to think it was doing that, you would have to put all these interactions in all the way up to the 25th order, which we didn't do. Again the right way to do it is to decide a priori --
Q. Which interactions should be looked at?
A. Yes.
Q. My question is: Have you reviewed an article where the author has gone through and used linear regression and sets forth in his article that before going forward and using linear regression here I've looked at my two groups and I've done propensity scores, or something at least equivalent to the propensity scores on them, not in just a main effects

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way but I've looked at a way in which I consider to be the important interaction effects?

A. Then the important interaction effects would have been in the original model if they thought they were important, if the author thought they were important.

Q. So they wouldn't need --

You are saying there wouldn't be a need to do a second analysis?

A. No.

Q. The main effects would include interactions because there would be interaction variables in the model?

A. Correct.

Q. All right. Have you reviewed an article where the author has done that, they used linear regression and they set forth a -- something similar to the propensity score analysis where they have shown what the B values and the R values are and for the variables, including interaction variables that they actually used in their model?

A. I have seen work that does that. Have I -- Can I refer to a specific article where that's actually part of the article? Quite possibly not. It may be somewhere, it may not be. The reason I'm saying that

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is, that's part of what you should do when doing the initial analysis of a data set, to see how you should analyze that data set.

When you publish articles, almost always the editors are pressing you to cut and reduce pages, and so background work that confirms the analysis you are doing is reasonably is often asked, don't do it, just say you did it. Do I know of any articles where they said they did it? Nothing comes to mind right away but I've seen articles in process where they do that kind of work to see how much overlap there is.

Q. If you were looking for articles that actually explicitly talked about doing propensity score testing before doing linear regression, is -- I think there is something called the current index to statistics that the Journal of American Statistical Association publishes. Is that a place where statisticians would go to look for something like that?

A. Correct, that's one place. You can also look in the article I wrote for Annals of Internal Medicine that was on propensity score methods, and I have a list of publications in there that are in medical journals primarily that used propensity score methods and I think more or less advocate them, I think some

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even provide software for them that have been written from the mid-'80s until now. I think the list of references may be 20 or 30 references.

Q. Do you know if the medical journals, people are using propensity scores in the articles, if that would show up in what's called a Medline search?

A. I've heard the Medline search and I think they probably would, so you can -- it certainly would be a straightforward thing to check. Let me clarify that. Just because it doesn't -- If an article did not use propensity score as a keyword, and you might not find it. It doesn't mean -- a double negative is going to be coming here -- it doesn't mean the authors aren't doing something intelligent to check the distribution of background variables and see how much overlap there is.

Q. I understand you are saying there is other ways of checking the background distributions other than using something that actually called propensity scores.

A. Correct.

Q. Have you reviewed articles before they are published where the authors are using linear regression of some type and they haven't said in the article or anything that they have told you about

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whether they have done anything like propensity scoring, whether it is actually called propensity scoring, to check the distribution of the background variables and where you -- have you questioned that author and said have you done this sort of analysis?

A. I probably have, yes. Let me -- let me be clear at the moment about the kind of articles that I do review as a referee for publications you are talking about now.

Q. Sure.

A. I primarily review articles that are regarded as needing my kind of review in the sense of relatively sophisticated statistically. The kinds of articles that are just straightforward applications of methods tend not to get sent to me anymore, and when I do get sent them, for a few years I've passed them on to students and other colleagues.

Q. Was there a time five or 10 years ago when you got more of those straightforward articles to review?

A. Yes.

Q. Going back to that time, did you receive such articles where authors using a regression don't set forth any explicit comparison of the background variable distributions, have you then required them if they want to get your approval and recommendation

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to go back and do such work?
A. I don't have a specific recollection of one. I have vague recollection of several times I would make a -- my report back to the associate editor and saying it seems to me this is -- this is a question that's an important question this person is addressing, that this comparison of the background variables between these two groups, see what the -- how the distributions compare, where they should be done, but it's up to the associate editor to make that kind of call. I would have made that kind of recommendation. I think it's essential.
Q. Do you know whether in any of those situations the article was published even though they never went back and did that sort of analysis?
A. I don't know but it wouldn't surprise me.
Q. That it was publish?
A. Yeah, articles like that are published and very often they are -- there are different kinds of criticisms, and the author kind of writes around them sometimes, maybe the author said he really did do that or maybe the author writes back and said I didn't do that but this is an important topic and I'm just following this other article you already published, why should I have to be held to a higher

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standard? There are all kinds of reasons why things get published, as I'm sure you are aware.

Q. In peer review journals you serve as a reviewer on?

A. In all peer review journals. There is no perfect article so there is always trade-offs that people make.

Q. If you turn to page 7 of your supplemental report. In the second paragraph there, you are looking at a subgroup composed of males 19 to 34 years old; is that correct?

A. Correct.

Q. In the last sentence of that paragraph, you say, "The statistical guidelines indicated in Table 1 mean that, in this situation, Zeger's regressions could grossly overcorrect or grossly undercorrect for bias." Did I read that correctly?

A. Yes.

Q. Now Professor Rubin, does Zeger's regression actually overcorrect for bias in that group?

A. Well we don't know, do we? We know what cell we are in in this table and since no one has ever looked to see at all what kind of relationships these outcome variables have to the covariates, we just don't know.

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Q. And --
A. We know the answer that's produced can't be trusted, that much we know.
Q. Do you know if in that group the Zeger regressions grossly undercorrect for bias?
MR. BIERSTEKER: Objection, asked and answered.
A. Yeah. What we know is the Zeger regressions cannot be trusted, they are completely unreliable.
Q. My question is: Do you know whether the Zeger regressions grossly undercorrect the bias?
MR. BIERSTEKER: Asked and answered.
A. We do not know what they are doing. They are basically tossing up coins and making answers.
Q. Can you give a probability that the Zeger model actually grossly overcorrects for bias in that situation?
A. Can I give a probability? This is kind of -- it's not a statistical. Is this kind -- you mean --
Q. I assume you haven't done any kind of calculation to give a probability; correct?
A. Correct, I haven't done any kind of probability because in fact in order to even try to understand whether the adjustment is doing anything sensible, you would have to look at the outcome variables, the

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Ys he is predicting from the Xs, and try to see what kind of relationship they do have to the background variables, the covariates, and the way they are being estimated, they are very -- they tend to be very noisy and so it would take care and I haven't done it. These analyses don't look at outcomes at all, they only look to background variables.

Q. So I take it you can't give me any sort of probability that it grossly overcorrects or grossly undercorrects in the 19 to 34 situation?

A. One hundred percent the way you record worded it.

Q. One or the other.

A. What I'm saying by that, I was being somewhat facetious, when you put the "or" in there, can you give me a probability that undercorrects or overcorrects, is the way I heard it -- maybe that wasn't what was said -- which means the probability is certain it does one or the other.

Q. And that's true for just about anything?

A. Everything where --

Q. Any time a linear regression, it is almost certain it does some overcorrecting or some undercorrecting?

A. Some little bit, yes.

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Q. Some little bit?
A. Not like this.
Q. My question really was, I meant to ask you, can you give me any probability that the Zeger model actually grossly overcorrects for bias in these males-19-to-34 situation?
A. I have no way.
Q. And the same for grossly undercorrect?
A. I have no idea. It's almost as if they didn't do the analysis.
Q. If you look at Display 1 on page 14 of your report. The first part lists the variables used in the main effects model.
A. Right.
Q. And I believe in your report you said there were 25 variables.
A. I think, yes, that's what I said.
Q. I want to make sure I understand, when I count the variables listed in parentheses I get 24, and would age then be the 25th variable? Is that the way to count the variables?
A. Yes, yes, that's correct.
Q. Have you made any examination of how those variables individually interact with smoking or with healthcare?

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A. As I said, I had not looked at the relationship, I have not looked at the relationship between these variables and any outcomes.
Q. Has anyone working for you or with you done that?
A. No.

(Plaintiffs' Deposition Exhibit 3560 was marked for identification.)

BY MR. LOVE:
Q. Professor Rubin, I'll show you what we have marked as Exhibit 3560. It's the supplemental report of William E. Wecker dated January 15, 1998.
A. Yes.
Q. You told me you had looked at that for the first time over the weekend in preparation for your deposition today.
A. Correct.
Q. And in reviewing that, did you reach any opinions about that report?
A. I -- My one opinion was that it was fairly clearly written, although rather sketchy in details. I also thought that the report appeared to make some -- some good points.
Q. Which were those?
A. The main point, the wrong question, has to do

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with the alleged misconduct, the same point I've been making, so therefore, tautologically, it's good; right?

Q. Yes.

A. According to me, otherwise it would be consistent. Studies the wrong population. I found that interesting, the idea that in NMES that there are people who are included there who are really not like the public aid population, and I haven't looked at that myself independently but I think it's an important criticism. I think I referred to that criticism earlier this morning when I was talking about what Harrison did in Oklahoma. I believe he used a different -- different definition than NMES.

Q. In your opinion, is it better to look at just the public aid population in NMES when you are estimating smoking-attributable expenditures for the Medicaid population of Minnesota?

A. For public aid people. In principle, yes. There is an issue of do the data become too thin and do you want to borrow strength from marginally relevant but not the same kind of people, and they haven't looked carefully enough to see that. But certainly it's better in principle to use just those subjects who are as close as possible to the subjects

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you want to reference, so in principle that's right.

Q. Have you seen any of the computer data that goes along with Dr. Wecker's supplemental report?

A. No, I have not.

Q. Did you find any other points that you agreed with in Dr. Wecker's report?

A. I -- I found the points of inconsistency relevant in the sense that the points pre -- inconsistent predictions across expense categories, across age and the results from the expenses from -- from males, what they point out is -- are things that give pause to the belief in the model. They don't make a lot of sense. So, they generate doubts about the model. The comments about -- I guess it's at the end here. Oh, the omitted information, I regard that as -- we already talked about this. Since I regard these kinds of analyses for smoking-attributable fractions and smoking-attributable expenses as descriptive in attempting to compare like with like as much as possible and the fact other people do include more Xs, more background variables in their models, then it certainly is true that you would like to include more information to make the comparisons of smokers and nonsmokers more comparable and more specific to a type -- particular type of person, so

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omitted-information criticism I think is a realistic criticism given the objectives, to compare like with like, accounting for such background characteristic.

These other facts he has about the inconsistencies from injuries and mental disorders gives pause to thinking about the kinds of analyses they are -- they are doing.

Q. Is that something, is that a way you have used to test models in the past, take something that was designed to measure smoking-attributable healthcare costs in a population and then see what it would calculate if you looked at just certain kinds of -- a subset of everything that you initially wanted to look at?

A. Well it's a -- it's a reality check and I think that the general advice to do so goes back for many, many years, to I think even work that was done on smoking a quarter century ago. When you try to get causal inferences from observational studies, one of the pieces of advice Cochran would talk about and I think he attributed to Sir Richard Dahl and Fisher, was because you don't have a randomized experiment, you try to make your causal hypothesis complex. This is very old advice. So that you -- What you should find is that if there really is a causal link, it

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should obey certain kinds of rational kinds of arguments. For example if smoking is bad for you -- I think this is a specific example used; I don't have the reference. But if smoking is bad for you in terms of lung cancer, then you should find people that smoke two packs a day have more lung cancer than people who smoke one pack a day, adjusting for other background characteristics.

Q. So the dose response situation?

A. Yeah, exactly right. Three packs a day should be worse than two packs a day.

Q. But this isn't a dose response that he is doing here.

A. No. I'll give you an analogy that came up in another study that I was involved in, private versus public schools, which are more effective at improving education, the scores, achievement of kids, and if you find out that the private schools, let's say, have a special math program. I'm making this up. The example isn't quite -- it's 15 years or more. If the private schools you are looking at have a special math program and then you do an analysis to find out whether the private schools do better than public schools at educating kids, adjusting for background variables which would include income of parents,

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education of parents, the age of the kids, incoming scores of the kids, that sort of stuff, and you look at the outcome variable math scores and you look at the outcome variable reading test scores, and if the public and private schools have the exact same reading program but they have a very different math program and then you find that this analysis shows there is no effect of private versus public on math scores but a big effect on reading scores, it doesn't hold together very well. The theory doesn't -- the theory that the schools are doing something new in math to improve performance doesn't hold together because it should be affecting math scores, not reading scores, so you don't believe the analysis.

Q. You think that's an analogy to what Dr. Wecker is describing --

A. Yes, I think it really is.

Q. -- at the bottom of page 5 and top of page 6.

A. It has that flavor, that's right. It has the flavor I will now use the same technology I used to estimate the effect of private versus public on math scores, I'll use it to look at reading scores. I might see just the same effect in reading scores when they have no special program in reading. That gives me pause to think I'm estimating the effect of the

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new math program. And so what I -- what I -- it's very -- it's very cursory, as you know, here, but what I -- reading this, basically he is doing something like that. He is -- he is doing an analysis that is smoking, you wouldn't think, would have. Do you see the analogy I'm trying to make here? The -- the lung cancer you see is like the math program that the new school has and the -- these conditions that aren't related to smoking, supposedly, poisoning, injuries, mental disorders, automobile accidents, are ones like the reading tests. Schools have no difference in programs in reading so why would you find a big effect for the public versus private schools on reading.

That's my analogy. Again, this is -- this is two or three sentences. I don't know exactly what was done but it has the flavor of that to me, so I think it agrees with the old advice that for more than a quarter century ago was used to actually look at these observational studies and smoking and cancer and try to draw some reasonable conclusions about directions. The results kind of hang together in some sense.

Q. If you look at the very next line, "'Attributable' Expenses Overstates Actual Expenses"

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on page 6.

A. Uh-huh.

Q. Did you come to any conclusions about whether that presents a problem that the attributable expenses due to six factors actually turns out to be more than the total expenses, or is that something you would expect to see in statistics if you use -- calculate attributable expenses to a whole variety of factors, that eventually you will get more than a hundred percent?

A. Sure. This is a criticism, I think, of thinking of these descriptive quantities as having something to do with causality, that you are sort of -- you are doing descriptive things and each of these are describing something in the population, assuming that you did the adjustment right, which I don't believe at all, the models do the adjustment right. What it's doing is it's sort of like that other checking point. You apply the same technology to something else and you get an answer that hangs together with the other answers if you are going to think of it as being a causal link. You sort of don't. But I have to see much more than this to really understand what's going on there, but the -- I think these things are reality checks and they are, I think, good

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points to the extent that I understand them from this very brief description. I think they are good criticisms in the same -- this very old sense of if you are trying to do something that's causal that you hang together, that the conclusion should hang together and make a consistent story, and these do not seem to do that very well.

Q. You don't believe that if you looked at any factors you can think of and ask whether those attributable -- whether costs incurred by healthcare program were attributable to age, sex, hair color, everything, if all the ones you took were positively associated with costs and you took 10 or 20 of them and they were fairly major ones, don't you expect that to be more than a hundred percent, since you didn't take any negative ones?

A. Yeah, typically I think you find to do that, that's right.

(Discussion off the record.)

(Deposition concluded at approximately 5:33 o'clock p.m.)

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C E R T I F I C A T E

I, David A. Campeau, hereby certify that I am qualified as a verbatim shorthand reporter; that I took in stenographic shorthand the foregoing deposition of DONALD B. RUBIN at the time and place aforesaid; that the foregoing transcript consisting of pages 475-735 is a true and correct, full and complete transcription of said shorthand notes, to the best of my ability; that the noticing party has been charged for the original transcript, and that ordering parties have been charged the same rate for such copies of the transcript.

Dated at Lino Lakes, Minnesota, this 14th day of April, 1998.

_____

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SIGNATURE PAGE

I, DONALD B. RUBIN, the deponent, hereby certify that I have read the foregoing transcript consisting of pages 475-735, and that said transcript is a true and correct, full and complete transcription of my deposition, except per the attached corrections, if any.

(Please check one.)

____ Yes, changes were made per the attached (no.) _____ pages.

____ No changes were made.

_____

DONALD B. RUBIN

Sworn and subscribed to before me this day of , 199__.

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Notary Public

My Commission expires: (DAC)

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